PHYSICS

OF THE

EARTH'S CRUST.

BY

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PREFACE.

For many years past I have been convinced that various questions of Physical Geology might be answered negatively, if not positively, by applying to them simple mathematical reasoning, and quantitative treatment. My own views have, in some respects, been greatly altered by the application of this method; and I confess that the present work contains within itself evidence of the circumstance; for not only will it be found that the views now put forward differ in some respects from those which I have previously published in contributions to scientific periodicals; but the effect of the progressive application of the quantitative method may be traced in the book itself; and the development of ideas, proposed in the earlier chapters, will sometimes be found to have taken an unexpected turn later on. This remark applies especially to certain hypotheses, which at first presented themselves in a favourable light, to account for compression and for the formation of ocean basins.

It is extremely probable that, if these investigations are carried further, some of the theories now offered as fairly established, may turn out to be untenable. With a growing science like Geology this is unavoidable. On a review of what I have written, I feel how many difficulties have been left unsolved, and some which have occurred to me not even mentioned. Nevertheless I hope that this attempt will not be without a certain value in advancing the study of the Physics of the Earth's Crust.

The mathematical reader will perhaps be surprised by the rough and ready mode of treatment adopted in some instances. But when it is recollected that, for the most part, we can assign only very hypothetical values for our symbols, it would be affectation to seek close results, which would after all have no greater value than those which claim to be only distant approximations.

The course of the argument and the general conclusions, will be found repeated in the Summary at Chapter XXII. But it is hoped that a perusal of this may not be substituted for reading the book. It is believed that the reasoning of the several chapters will be found intelligible, even without wading through the calculations.

My thanks are due to the Councils of the Geological Society of London, and of the Philosophical Society of Cambridge, for permission to make free use of papers contributed by me to their publications. I have similar acknowledgments to make to the Proprietors of the *Philosophical Magazine*, the *Geological Magazine*, and the *Quarterly Journal of Science*. My kind friends, H. W. Bristow, Esq., F.R.S., Senior Director of the *Geological Survey*, and A. F. Griffith, Esq., B.A., of Lincoln's Inn, Scholar of Christ's College, Cambridge, have rendered me most valuable help in revising the proofs while they were passing through the press.

OSMOND FISHER.

Harlton Rectory,
18 *October*, 1881.

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ERRATA.

Page 33, line 8, *read* 171·875.
,, 46, ,, 3, for "l" read "$l(1+e)$".
,, 75, ,, 14, *for* "one" *read* "one or both".
,, 180, ,, 6, *for* "obtaining" *read* "obtained".
,, 182, ,, 4 from bottom, *for* "μ" *read* "σ".
,, 219, ,, 2, of note, *for* "crust of" *read* "crest of".
,, 244, ,, 5, *for* "lava" *read* "magma".
,, 273, ,, 17 (in some copies) *for* "mean" *read* "datum".
,, 278, ,, 3 from bottom, *for* "not so deep as" *read* "deeper than".

CHAPTER I.

ON UNDERGROUND TEMPERATURE.

The rise of temperature on descending into the earth—The causes which have been assigned for it—Attempts to determine the law of increase by observation—Their results—Deep bore-hole at Sperenberg and methods of observation used there—General effects of convection currents in a bore-hole—Tabulated observations at Sperenberg—Anomalous result supposed to be deducible therefrom—Anomaly explained—General law of average rise of temperature not impaired thereby—Hot springs and volcanos prove that the rise of temperature continues, but do not inform us whether the law near the surface obtains unaltered for all depths.

THERE is no fact more firmly established in terrestrial physics than that the temperature of the rocks of the earth's surface increases with increasing depth. Observations upon this subject have been made in all parts of the world, and the same result has been everywhere arrived at. Even in the frozen soil of Yakoutzk, in Siberia, this increase of temperature is found to prevail, although the ground is congealed to the whole depth penetrated[1]. In all mining operations this gradual increase of temperature becomes a very serious consideration, rendering human labour at great depths a very severe trial to the constitution of the workman. And

[1] The increase of temperature at Yakoutzk, although the soil is frozen even beyond the depth reached, proves that this freezing is owing to the present low mean temperature of the locality, and that it is not a residual effect of a former glacial and still colder period; for if that were so the strata would be now warmer above than below, whereas the reverse is in fact the case.

The average temperature for the year is −8·6, R. or 12·7, F. Prof. Milne, in "Geol. Mag." N. S. Vol. IV. p. 518.

indeed it is this circumstance, more than any other, which fixes a practical limit to the depth at which mining operations are possible. It is not the raising the minerals from profound depths, for the resources of modern engineering are quite competent to overcome any difficulty on that score; it is not keeping the mines clear of water, for they are less troubled with its influx at great than at moderate depths; but it is the impossibility of furnishing the men with an atmosphere to breathe in below the temperature of the blood. For when the air has to be conveyed to long distances it acquires the temperature of the rocks, and no means have been yet suggested which could furnish it at the atmospheric temperature unaffected—or but slightly so—by its long journey through the subterranean passages.

This increase of temperature, though universal, is not everywhere the same. The average is about 1 degree Fahr. for between 50 and 60 feet of descent. Such is the result of very numerous observations. Some of these have been made by drilling holes in the rock in deep mines; others by lowering thermometers in Artesian bore-holes. A Committee was appointed by the British Association to report upon the subject, and their reports extend from the year 1869. Much information upon the matter may also be gathered from the Reports of the Parliamentary Commission upon the Coal Supply.

Unquestioned as is the fact, nevertheless the cause of this increase of heat has formed the ground for much speculation. The most obvious explanation of it is offered by the phenomena of hot springs and volcanos. These show us that, in some places at any rate, much higher temperatures exist at great depths than have ever been reached by artificial perforations. But these phenomena might be said to be local, while the general slow increase of temperature we have been describing exists more or less at every place. Are these phenomena directly connected? Are they indirectly connected? Or, are they altogether unconnected? Various answers have been returned to these questions.

Among "practical men" engaged in mining operations an opinion seems to have prevailed that the increase of temperature

in deep mines is due to the pressure of the overlying strata or "cover." We may unhesitatingly dismiss this hypothesis. Pressure by itself cannot develop heat. Where motion is destroyed as motion, there it is that it is converted into heat. Thus the motion which is destroyed when a hammer strikes upon an anvil will develop heat that can explode fulminating powder, or heat a nail red-hot. So, also, the bearings of a wheel become heated by friction, which gradually destroys its motion. But though mountains rise on mountains, there will be no heat produced unless motion of some kind is destroyed. In deep coal-mines an effect called "creep" is caused by the enormous pressure upon the sides of a passage, or upon the pillars left to support the roof, causing the floor of the excavation to swell up. In this case there is immense friction between the particles of the rock, were it not for which the passage would become instantaneously closed. It has been remarked that considerable heat sometimes accompanies this creep, in which, be it observed, we have not pressure alone, but motion destroyed by friction and converted into heat.

Sir Humphry Davy ascribed the volcanic fires to the oxidation of earthy and metallic bases, supposed by him to exist low down in the interior of the earth. Since water is everywhere present in the crust, if it be dissipated by the abstraction of its oxygen when it encounters the bases, and by the blowing-off from volcanic vents of the equivalent hydrogen, then its place must be continuously supplied by the percolation downwards of fresh accessions; and thus a generally diffused increase of temperature was supposed to arise, and to manifest itself by its escape towards the surface.

A very fascinating theory, which has met with support among many scientific men, has of late years been promulgated by Mr Mallet, who considers the heat of volcanic action to be derived from the transformed work of crushing the rocks of the earth's crust, owing to the contraction of its interior through long-sustained cooling. The cause of evolution of the volcanic heat under this theory is of the same kind as that alluded to above, in the case of creeps; while the ubiquitous increase of temperature in descending into the earth is looked

upon as chiefly a manifestation of the generally diffused heat of the interior, by the escape of which the contraction in volume is produced. Volcanic action, Mr Mallet tells us, is caused by this contraction manifesting itself locally and paroxysmally.

In order to decide what theory best accounts for the phenomena, it is obvious that the first step is to make sure of the facts themselves. In other words—What is the law regulating this increase of heat? At what rate does the temperature augment? This might be supposed to be a point easily settled. It might be supposed that nothing would be easier than to insert thermometers into the rock at different depths in a mine, and to read off their indications; or to lower them into a bore-hole for the same purpose. But there are many difficulties to be overcome, and a host of disturbing causes present. The rock, or the face of the coal in a mine, is affected by the temperature of the air. The air is warmed by the presence of men and horses; it is cooled by the ventilation carefully kept up. Hence the surface of the working is not at the true temperature of the rock. The result of careful experiments made by Sir G. Elliot showed that when thermometers were inserted in the coal in a long-wall working, at distances of 3, 6, and 12 feet from the face, no alteration could be detected in the indications given beyond 3 feet from the face of the coal; and his observations were accordingly taken at 4 feet. It is evident, as will be seen by what will be said hereafter, that the necessary distance must be governed by the difference between the temperature of the ventilating air and of the seam, and that the greater this difference the farther it would be necessary to bore into the coal to obtain an estimate of its true temperature. If it be attempted to ascertain the law of increase by means of an Artesian bore-hole, there are here also disturbing causes present. The action of the tool warms the rock by mechanical means, the friction or pounding action developing considerable heat. Hence while the work is in progress no reliable observations can be taken, except during intervals of suspension, and then it appears—as will be seen further on—that the interval needs to consist of weeks rather than of days. When the work is completed, and the bore stands nearly full of water, it is the tem-

perature of the water which is obtained on lowering a thermometer into it, and we cannot be sure that that coincides with the temperature of the rock. In fact many reasons may be assigned why it should not do so. Springs may enter on one side and flow out at the other; or they may rise from lower levels and flow away at higher. The very act of lowering the thermometer tends to mix up the differently heated layers of water, and to confuse the result.

A single instance will suffice to illustrate the irregularity of increase referred to. At Rose Bridge Colliery the temperatures were taken by drilling a hole a yard deep at the bottom of the shaft during the process of sinking. If the mean temperature of the surface there be taken at 50° F., the mean rate of increase for the whole depth was 1 degree for 55 feet. But at the successive depths of 605, 630, 663, 671, 679, 734, 745, 761, 775, 783, 800, 806, 815 yards respectively the rate appears to have been 1 degree for 70, 25, 49, 24, 24, 110, 66, 32, 42, 48, 51, 36, 54 feet.

Within the last few years a very deep boring has been carried down at Sperenberg, near Berlin. This has reached the extraordinary depth of 4052 Rhenish feet, or 4172 British feet. A most surprising geological fact about this boring is that, with the exception of the first 283 feet, which were carried through gypsum with some anhydrite, the remainder passed entirely through rock salt. This seemed to offer an exceptionally good opportunity for observing temperatures in a homogeneous rock, which might be expected to be comparatively free from the sources of error arising from variable heat-conducting power in the layers successively penetrated[1]. The observations

[1] If F be the flow of heat from below, κ the conductivity of the rock, v the temperature, and x the depth, then

$$F = \kappa \frac{dv}{dx}.$$

Now the flow of heat at depths beyond the reach of the influence of the seasons will be the same for moderate depths, or F is constant, and $\frac{dv}{dx}$ gives the increase per foot of descent.

Hence $\frac{dv}{dx} = \frac{F}{\kappa}$ shows that the increase of temperature per foot is inversely proportional to the conductivity of the rock, so that where the rock conducts heat badly the increase will be more rapid, and *vice versâ*.

in the bore were taken with much precaution, under the direction of Herr Eduard Dunker, Inspector of Mines. A *résumé* of his paper upon them will be found in "Nature," Vol. XV. p. 240, 1877, as contained in the "Ninth Report of the British Association Committee on Underground Temperature." In Vol. XII. p. 545 of the same publication (1875) there had previously appeared a notice of a contribution by Prof. Mohr, of Bonn, to the "Neues Jahrbuch für Mineralogie" (1875), in which that gentleman had commented upon the law of increase which he supposed to be deducible from the observations made in this bore-hole. The result at which he arrived was remarkable enough, and, if it could not have been explained away, would have justified the conclusion which he drew from it, which was that the source of heat within the crust of the earth was situated within the crust itself, for that after a certain depth was reached—which he put at 5170 feet—the increase would be *nil*. However, in the Report of the British Association just published, it is explained how this result had been arrived at, and it is distinctly shown that Prof. Mohr's conclusion was in reality not based upon the original observations themselves, but that it arose from the form of an empirical mathematical formula, representing a parabola, which Dunker had assumed to express the law. In fact it had been implicitly assumed that the increase would come to an end, and all that Mohr did was to find out at what depth it would do so, supposing that assumption true.

This bore-hole, from the favourable circumstances already referred to, and the care with which the observations were made, deserves full consideration. The temperatures were taken in two manners. In one set of observations the temperature of the water in the bore-hole was observed with a suitable thermometer; in the other an apparatus called a geothermometer was lowered, which cut off a portion of water from that above and below it by two disks or bags. A thermometer was enclosed in the space between the disks, and, after the thermometer had been down not less than ten hours, the temperature shown by it was assumed to be that of the rock at the corresponding depth. This was found to differ sometimes by

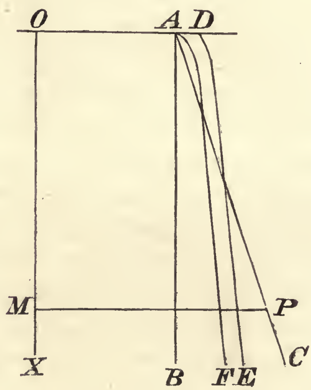
more than a degree Réaumur from the temperature of the water at the same depth, as shown by the first-named thermometer, when the water above and below was not cut off. Now, it can hardly be supposed that so great a difference as this really existed simultaneously between the surface of the rock exposed to the water in the bore-hole and the water itself. On the contrary, it seems evident that the surface of the rock, being exposed to the action of the water, must have assumed the temperature of the water, whatever that might be, at any given depth. But the water was undoubtedly affected by convection currents, and consequently its temperature was not the same as that of the rock *in mass* at a distance from the bore-hole. Hence, when the circulation of these currents was stopped by the disks, the surface of the bore-hole would begin to tend towards the true rock temperature. If the water at any depth was warmer than the rock in mass, its temperature would begin to fall when the currents were cut off, and if the water was cooler than the rock it would begin to rise.

Let us, then, shortly consider the general effect of convection currents as they would affect the water.

These currents arise from the circumstance that when water is warmed it expands, and consequently becomes lighter. This expansion is exceedingly small; yet in a substance of such extreme mobility as water it is sufficient to cause the expanded portions to rise, while the denser portions above sink to supply their place. The expanded portions in rising carry their heat up with them, and the cooler portions in sinking reduce the temperature of the lower part of the column. If therefore we had a column of water originally warmed towards its lower portion, but not supplied there with fresh accessions of heat, the whole would, if open to the atmosphere, shortly assume an equable temperature throughout.

Let us now invoke the aid of a diagram to render our ideas more clear; and suppose the depths in the bore-hole to be measured along the vertical line *OX*, and let lines drawn at right angles to this represent, in proportion to their lengths, the temperatures at the corresponding depths. Let *OA* represent the mean temperature of the surface of the ground. Then

on the supposition that the temperature of the rock in mass increases proportionally to the increase of depth, the temperature of the rock will be represented by a *straight* line, as *AC*. If, then, the temperature of the water in the bore-hole correctly gave the temperature of the rock, its temperature would be likewise represented by the same straight line *AC*. But since it is affected by convection currents this cannot be the case; for they tend to warm the upper portions of the column of water, and to cool the lower. Hence, so far as we have gone, the water in the upper part of the bore-hole will be warmer than the body of the rock, and in the lower part it will be cooler; and in our diagram we must draw a line to represent its temperature, more distant from *OX* in the upper part, and nearer to it in the lower. It need not, however, be a straight line, the law of increase of temperature being possibly altered. Suppose, then, *DE* to be this line, or the curve of temperature of the water on the supposition now made. It will be seen that it must intersect the line *AC*.



There is a further consideration to be taken into account. If the bore-hole is nearly full of water, and of considerable dimensions, it will present a considerable surface to the atmosphere. At Sperenberg the water stood seven feet from the stage of the bore-pit, and the bore-hole was a foot in diameter.

The consequence would be that the open surface of the water would be cooled, sensibly to the temperature of the air. This would bring the curve of temperature of the water at the surface nearly to the same point as the temperature line of the rock there, and it would also have the effect of reducing the temperature of the water throughout the column, below what it would be if it had not this extrinsic cause of cooling. The ultimate result would be that the temperature curve of the water would assume some such form and position as *AF*. It will be observed that this line also intersects *AC*; and the signification of this is, that the water in the bore-hole is warmer than the rock mass in its upper portion, and cooler than the rock mass in its lower portion; or, in other words, the water tends to warm the rock in the upper portion, and to cool it in its lower portion. Consequently, if the circulation of the currents be interrupted, the water imprisoned between the disks of the geo-thermometer would in the upper portion of the hole gradually become cooler, as heat was conducted away from it into the body of the rock; and it would gradually become warmer in the lower portion, as heat was conducted into it from the body of the rock. But at the particular depth at which the two lines *AC* and *AF* intersect no alteration would take place.

That such an effect was actually produced is shown by the following table, the first two columns of which are taken from Dunker's Table II., in his paper entitled "Ueber die Benutzung tiefer Bohrlöcher zur Ermittelung der Temperatur des Erdkörpers, und die desshalb in dem Borloche I zu Sperenberg auf Steinsalz angestellten Beobachtungen." The two temperatures marked with asterisks differ from those quoted in the British Association Report from Dunker's quarto paper of 1872 in the "Zeitschrift für Berg-Hütten und Salinen-Wisen." But they are printed as here given in the octavo paper from which they have been copied, and are stated there to be the mean results of several observations. Dunker considered the deepest observation with the geo-thermometer unsatisfactory.

The difference between a Prussian and an English foot is so small that, for the purpose in hand, they may be considered equal.

Depth in feet.	Temp. R. Water shut off.	Temp. R. Water not shut off.	Water shut off, consequent alteration of Temp.	Surface Temp. 7·18°. Absolute Increase at each depth in Col. 2.	Being at the rate of 1° R. per No. of feet.	Absolute Increase at depths.	Being at the rate of	
							1° R. per ft.	1° F. per ft.
15	9·40	10·35	−0·95	—	—	—	—	—
30	9·56	10·20	−0·64	—	—	—	—	—
50	9·86	10·40	−0·54	—	—	—	—	—
100	10·16	12·30	−2·14	2·98	33	—	—	—
300	14·60	13·52	+1·08	4·44	45	—	—	—
400	14·80	14·30	+0·50	0·20	500	—	—	—
500	15·16	14·68	+0·48	0·36	277	—	—	—
700	17·06	16·08	+0·98	1·90	105	—	—	—
900	18·50	17·18	+1·32	0·44	455	—	—	—
1100	*19·90	19·08	+0·82	1·40	143	11·72	94	42
1300	21·10	20·38	+0·72	1·20	166	—	—	—
1500	22·80	22·08	+0·72	1·70	118	—	—	—
1700	24·10	22·90	+1·20	1·30	154	—	—	—
1900	25·90	24·80	+1·10	1·80	111	—	—	—
2100	*27·70	26·80	+0·90	1·80	111	7·80	128	57
2300	28·50	28·10	+0·40	0·80	250	—	—	—
2500	29·70	29·50	+0·20	1·20	166	—	—	—
2700	30·50	30·30	+0·20	0·80	250	2·80	214	95
2900	—	31·60	—	—	—	—	—	—
3100	—	32·70	—	—	—	—	—	—
3300	—	33·60	—	—	—	—	—	—
3390	36·15	34·10	+2·05	5·65	122	—	—	—
3500	—	34·70	—	—	—	—	—	—
3700	—	35·80	—	—	—	—	—	—
3900	—	36·60	—	—	—	—	—	—
4042	38·25	38·10	+0·15	2·10	310	7·75	212	95

In the above table the first column gives the depths in Prussian feet, at which the observations of temperature were made.

The second column gives the temperatures observed at the respective depths when the convection currents were stopped by the use of the geo-thermometer.

The third column gives the temperature of the water in the bore-hole when the convection currents were allowed to circulate.

The fourth column gives the alteration of temperature at each depth which arose from stopping the convection currents—*minus* when the temperature fell, *plus* when it rose.

The fifth column gives the increase of temperature at each depth over that at the previous one, as taken from the second column.

The sixth column gives the rate of increase at each depth, measured by the number of feet which it would be required to descend to gain an increase of 1 degree Réaumur,

on the supposition that the increase per foot remained constant for that number of feet; consequently large numbers show a proportionately slow increase.

The seventh column, like the fifth, shows the increase of temperature at the depth against which the number stands, over that at the depth at which the previous number stands; the object of this column being to obtain averages at longer distances apart.

The eighth column, like the sixth, shows the rate of increase of temperature with longer averages, measured by the number of feet of descent for 1° Réaumur.

The ninth gives the same increase when measured in feet of descent for 1° F.

In discussing this table the first point to be noticed is that the rate of increase is by no means so equable as, from the homogeneity of the rock, it might have been expected to have been. In order to obtain anything like a general law of increase, it is necessary to take the average of the increase at considerable distances apart.

The second point has been already adverted to, viz., that the shutting-off of the convection currents caused a diminution of temperature in the upper part of the bore-hole, and an increase of it in the lower. This shows clearly that the temperature of the rock was altered temporarily, by the action of the convection currents, to some distance away laterally from the bore-hole. The hole was lined with three tubings, one behind another, for the upper 440 feet, and, on account of the presumed convection currents still existing behind the tubing, the observations in the upper part of the column were not considered trustworthy. But this circumstance does not militate against the conclusion at which we have arrived, because whatever effect convection currents might have upon the temperature would be *pro tanto* produced by these concealed currents, so that all they could do would be to lessen the effect of shutting off the currents in the main channel. And we may reasonably suppose that the change of temperature arising from that proceeding would have been still greater than recorded, if the tubing had been in close contact with the rock.

In the discussions of these observations[1] the observations made with the geo-thermometer have been in general looked upon as giving a near approximation to the temperature of the earth's crust at this locality; and, consequently, it is these which we propose to consider further. Although the sixth column shows that the rate of increase of temperature was far from equable when comparatively short intervals are taken into account, yet, when we pass to the eighth and ninth columns, we observe that on the whole there is a decided diminution of the rate of increase in the lower depths. It was probably this circumstance which induced M. Dunker to assume that empirical formula for the law of increase which led Prof. Mohr to believe that, at the depth of 5170 feet, the increase would be *nil*, and thence to conclude that the source of the heat of the crust must be situated within the crust itself, instead of—as is usually supposed—coming up from the profound depths below. The question which presents itself therefore (and it is a most important question) is—Can this diminution of the rate of increase in the indications of the geo-thermometer be consistent with an equable rate of increase in descending as deep as the observations went into the earth's crust? The answer, that it can, seems to follow from the considerations already made on the effect of convection currents upon the temperature, not of the water only, but of the rock itself. It is obvious that the continued contact of water at a different temperature from that of the rock must alter the temperature of the rock itself where it is in contact with the water. And it has been remarked that, almost beyond dispute, it had actually that effect; consequently the rock in contact with the water must have been cooled in the lower part of the bore-hole. Moreover, the principal cooling effect of the currents would occur towards the bottom of the hole, where the cold water coming down from above would tend to accumulate, because there would be no warm currents coming up from below to disturb it.

When, therefore, we enquire how far the observations

[1] The writer has met with a course of lectures entitled "Vorträge über Geologie, von F. Henrich, Wiesbaden, 1877," in which the Sperenberg observations are discussed with much acumen.

made with the geo-thermometer can be depended upon as having given the true temperature of the rock in mass, two questions present themselves. First, could the water enclosed between the disks assume eventually the temperature of the rock mass, if the instrument were left down long enough? And, secondly, if it could do so, was it in fact left down long enough?

It appears that this result could be only partially attained, however long it were left in the bore-hole. The convection currents in the water would affect the temperature of the column at the upper and under surfaces of the enclosing disks of the instrument. And even if the disks were purposely made of such badly-conducting material as to prevent any temperature effect from the currents being carried through the disks, still such effect would be conducted round the edges of the disks, through the substance of the rock itself; consequently, however long the instrument might have been allowed to remain down, the temperature of the water enclosed between its disks would have been to some extent affected by the convection currents; and their tendency in the lower parts of the column would be to reduce the temperature, and diminish the rate of increase.

A method of obviating this source of error is mentioned in the British Association Report, already referred to, as having been lately suggested by Sir Wm. Thomson. It consists in using a series of india-rubber disks placed at considerable distances apart.

Putting the above-discussed source of error aside, we come to our second enquiry, whether the geo-thermometer was left down long enough for the water between its disks to assume the temperature of the rock mass[1]? Let us, then, consider the conditions of the system before the geo-thermometer is introduced. We have a very long vertical column of water

[1] It appears that at the depth of 1100 feet an observation of ten hours duration gave the same result as one of nineteen hours; whence it was concluded that ten was long enough. But considering the great thermal capacity of water, and the small changes which, except just at first, might be expected in the rock, as well as the difficulty of the operation, this trial can hardly be held conclusive against the probability of a further increase of temperature, if the geo-thermometer had remained down longer.

enclosed in a cylindrical hole within a mass of rock, the rock extending to an infinite distance both sideways and downwards. The water in the bore-hole at any given depth has, in consequence of the currents, a temperature which differs from that of the rock on the same horizon at a distance from the hole. This temperature of the water may be higher or lower than that of the rock; but we will suppose it lower, as it will be in the deeper parts of the hole. The rock surface of the bore-hole, being constantly laved by the water, has been brought to the same temperature as the water. It necessarily follows from this that if we could examine the temperatures of the rock at greater and greater distances laterally from the bore-hole, we should find them become higher and higher, and we should have to penetrate to some considerable distance before the increase came to an end, and when it did so we should feel assured that at last we had reached rock of the true temperature for the depth. The greater might be the difference between the temperatures of the water and the rock in mass, the further we should have to penetrate for that purpose, and this would be furthest in the lower portions. Now suppose the currents interrupted by lowering the geo-thermometer, so that the heat ceases to be conveyed away from the rock surface of the bore-hole; and we will now dismiss the consideration of the effect of the water above and below the disks of the instrument. The heat which flows out of the sides of the bore-hole into the imprisoned water is now no longer conveyed away by the currents, and begins to accumulate there. The rock in the neighbourhood of this water begins to get warmer. The region of the true rock temperature approaches nearer and nearer to the bore-hole, until at last it reaches it, and then, and not until then, the imprisoned water assumes the true temperature of the rock. This process will require time, and that a long time. In the first place, on account of the great capacity of water for heat, it requires much more heat to warm up a volume of water through a given range of temperature than it would require to warm up an equal volume of rock. In the second place, it requires a long time for the heat to travel through the rock to reach the water. And although the conductivity of rock salt is said to be considerably higher than that of

ordinary rock, so that the heat would pass more quickly through it[1], yet there will be a compensating effect in the present instance, because, on account of the greater conductivity, the true rock temperature would not be reached within so small a distance from the bore-hole, so that the heat would have a longer distance to travel before the true temperature could be restored. That the geo-thermometer was not left in place sufficiently long for this purpose seems to be proved by the following instance of the extreme slowness with which the passage of heat under such circumstances takes place. From comparing the British Association Reports for 1872 and 1873 we gather that, in the course of the observations made at the Artesian well of La Chapelle, at St Denis, it was noticed that the temperature at the bottom of the hole was much affected by the action of the "trepan" or boring tool. It appears to have been expected that this effect would have passed off in a few days. Accordingly, a week after the tool was stopped, observations were taken, and there was found what was thought to be an unaccountably sudden increase of nearly 8° F., in 60 feet of descent, near the bottom of the hole. But in the following year a second set of observations were made "some months" after the boring had been suspended, and the abnormal increase was found to have entirely disappeared. We learn, then, that *a week* was not sufficient for the rock wall of the bore-hole to regain its balance of temperature after having been disturbed through about 7° F. How much longer was required we have not the means of knowing. It seems a necessary inference that a few hours would be quite insufficient for a correct observation, and the time allowed at Sperenberg did not exceed such an interval. The conclusion must therefore follow that, on both the accounts referred to, the geo-thermometer must have shown in the lower depths a temperature less than the true temperature of the rock of the earth's crust existing at that depth.

The reasons given above seem quite sufficient to account for the diminished rate of increase of temperature shown in the table; and they would lead to the conclusion that in all observations made in bore-holes of a sufficient width to allow

[1] This however is doubtful, for if it were so, the rate of increase ought to have been less than the normal rate. Such was not the case. See note, p. 5.

convection currents full play, the temperatures taken at the lower parts will—even with the geo-thermometer—be less than the true rock temperature. The conclusion appears a legitimate one that the diminution in the rate of increase in a bore-hole, even when the convection currents are temporarily shut off, does not necessarily imply that there is any such diminution of the rate in the body of the rock; or, in other words, that the temperature curve within the rock may be a straight line, although that given by the geo-thermometer be concave to the axis of depths. It will also follow that the mean rate of increase for the whole depth within the rock will exceed that shown by the geo-thermometer. The mean increase, for instance, in the above table, between the temperature of the surface and that at 4042 feet, is 1° R. for 129 feet, or 1° F. for 57 feet; and it is stated in the Association Report that, when the observations are corrected for pressure, the mean rate to 3390 feet is 1° F. for 51·5 English feet. This is about the usual average. We may therefore conclude that at Sperenberg the actual rate of increase in the body of the rock is greater than this.

The most important conclusion from the above is that, in spite of the decreasing rate shown at Sperenberg (and also at St Louis, and perhaps other places), the old assumption is probably correct, that for such depths as have been reached, the rate—if it could be truly observed, and setting aside local causes of disturbance—is probably a uniform rate, amounting to, or, as the above reasoning would lead us to infer, rather exceeding, 1° F. for 51 feet of descent. And this is all that we know experimentally respecting the *rate* of increase of temperature within the earth. We have reason, however, to believe that at still greater depths the temperature continues to increase, because many thermal springs deliver water at the surface at a much higher temperature than has been reached by any artificial perforations. And yet the waters yielded by Artesian wells are in their degree thermal springs, so that the hotter natural springs, such at least as are not obviously situated in volcanic regions, are probably exactly in the same category as the deeper Artesian wells. For instance, it appears extremely probable that the Bath waters are simply springs supplied by the rainfall on the Mendip hills. The water following the sub-

terranean course of the limestone is kept down by the impervious beds of the basin-shaped coal-measures, and reascending to the surface along the fault from which the springs are known to rise, brings with it the temperature of the rocks through which it has percolated. The temperature is 120° F., which is 70° above what may be taken as the mean surface temperature, and would require, at the rate of one degree for fifty feet, a depth of 3500 feet to give the required increase. This is by no means an unlikely depth to be attained by the synclinal trough of the carboniferous limestone in Somerset.

But the phenomena of volcanos, which are situated in all regions of the globe, and of which relics remain almost everywhere among ancient rocks, point to an intensely high temperature at still greater depths. They afford however no clue as to what the depth may be from which their liquid ejectamenta are derived without first assuming a law of temperature.

Thus, it is a most important point to be borne in mind, that we *know nothing by observation* respecting the law of increase of temperature within the earth, beyond the facts that *near the surface* the average rate of increase is 1° F. for between every 50 and 60 feet of descent, and that the temperature at greater, but unknown, depths is very high, and sufficient to fuse what, from that very cause, we term igneous rocks. This temperature cannot be lower than between 2000° and 3000° F., and may be much higher.

But if an average rate of increase of 1° F. for every 60 feet were to continue to all depths, the temperature in the interior would become enormous. In fact, at the depth of from 200 to 400 miles it would amount to from 18000° to 36000° F., which is what Prof. Rosetti has estimated from his experiments with the thermopile to be the probable temperature of the sun[1].

[1] His words are these: "I think then that I may fairly conclude that the true temperature of the Sun is not very different from its effective temperature; and that it is not much less than *ten thousand degrees* [cent.] if we only consider the absorption of the terrestrial atmosphere; nor much more than *twenty thousand degrees* if we also take into consideration the absorption by the solar atmosphere, estimating the latter at $\frac{88}{100}$ of the total radiation of the Sun." "Phil. Mag." Vol. VIII. Fifth series, p. 550.

CHAPTER II.

CONDITION OF INTERIOR.

Little known about the condition of the interior of the earth—Results of the mathematical "Theory of the Earth" point to a former condition of fluidity—Determination of the mean and surface densities—The question proposed whether the fluid condition still partially exists more or less—Connection between this question and that of the law of internal temperature—Two modes of attacking the question, (1) *Precession,* (2) *the tides—Opinions of Sir Wm. Thomson, Hopkins, Delaunay, and others—Possibility of a cooled crust resting on an internal fluid layer.*

VARIOUS suppositions may be made respecting the condition of the interior of the earth. What we actually know about it is very little. Whether any portion of it, or how much of it, is at the present time liquid can only be determined by secondary considerations. But we may feel almost certain on account of its present form and the law of variation of gravity upon its surface that it was once wholly melted, and that it consists at present of concentric spheroidal shells, each of equal density throughout, and of definite form; such form having been determined by the velocity of rotation and law of density, coupled with the law of mutual attraction of all the mass. The ellipticity of these shells decreases from the surface towards the centre. The mean density of the surface has been estimated at from 2·56 to 2·75. The mean density of the whole is nearly twice as great. The summary of the experiments from which these constants may be determined is as follows:

1. Maskelyne, 1775, by means of the deflection of the plumb-line by the mountain Schehallion determined the mean density of the earth at 5·0.

2. Cavendish, 1798, by the torsion rod and two leaden balls as attracting masses, weighing together 700 pounds, in 23 experiments determined the mean density at 5·448.

3. Reich, 1837, using one large leaden ball of 99 pounds for the attracting mass, in 57 experiments determined the mean density at 5·44.

4. Baily, 1842, using two large leaden balls, each weighing 380½ pounds, in 2004 experiments determined the mean density at 5·67.

5. Airy, 1856, by experiments with the pendulum in Harton Colliery, at a depth of 1260 feet, determined the mean density of the earth at 6·566. The mean density of the ordinary beds of rock above was determined by W. H. Miller at 2·56, and of the coaly beds at 1·43. Airy considered that his determination may "compete with the others on at least equal terms."

6. Col. James, 1856, by observations on the deflection of the plumb-line at Arthur's Seat determined the mean density of the earth at 5·316. The mean density of the rocks at Arthur's Seat was determined at 2·75.

7. Mallet estimates the weight of a cubic foot of granite at 178·34 pounds, which makes its density 2·69.

8. Waltershausen finds the specific gravity (or density) of

Albite	2·52
Quartz	2·62
Limestone	2·63
Mica	2·92
Mean	2·66

which he takes as the mean density of surface rock. Such a mean, however, would require these minerals to occur in equal proportions in the rock.

We have then for the mean densities according to the experiments respectively of

	Whole earth.	Surface rock.
Maskelyne............	5·0	
Cavendish	5·448	
Reich	5·44	
Baily...................	5·67	
Airy and Miller	6·566	2·56
James	5·316	2·75
Mallet		2·69
Waltershausen.......		2·66

If we select Col. James' determination for the density of surface rock, viz. 2·75, and double it, we get 5·50, which is a fair representation of the mean value of the density of the whole earth; being somewhat greater than the mean 5·375 if Airy's determination is omitted, and somewhat less than the mean 5·573 if it be included.

These are the values used by Archdeacon Pratt in his "Figure of the Earth."

The increase of density in approaching the centre will depend partly upon the pressure, and partly upon the nature of the materials. We do not know enough about the ultimate constitution of matter to reason satisfactorily about the effect of pressure. It has been thought that by that means the density of a substance can be increased up to a certain limit, but no further. If this be so, it renders it probable that the high density of the central parts of the globe is due to the intrinsic nature of the materials there, and that a large proportion of the heavy metals occupy that position.

The assumption of a law of density for the purpose of determining the forms of the strata of equal pressure and density within the earth will also give a corresponding form for the outer surface. And it is found that a law (due to Laplace and suggested by analytical necessity) which, using the mean values for the surface and mean densities indicated above, leads to an ellipticity of $\frac{1}{293}$ for the outer surface, is consistent with the supposition that the density, if it depended on pressure alone (without asserting that it does so, which has been shown above to be highly improbable), would be such, that the increase of the square

of the density would vary as the increase of the pressure[1]. Now the actual ellipticity being known by measurement to be $\frac{1}{295}$ it is highly probable that the law of density above indicated expresses a close approximation to the truth.

The conclusions here enumerated are the results of remarkably refined mathematical investigations which have exercised the ingenuity of the greatest masters in the science, and the methods and results of which will be found collected in the work by Archdeacon Pratt just referred to.

To guide us in our reasoning concerning the condition of the interior we have then the following data:

The observed rate of increase of temperature near the surface is perfectly compatible with the existence of such a temperature within the earth as would fuse all known substances, and fused substances continue to flow up from below, and, what is more, there is almost positive evidence that the earth was at one time wholly fluid. The question arises, does this fluidity still normally exist within it, wholly or partially?

The true law of increase of temperature is inextricably mixed up with this other question of the condition as to solidity or fluidity of the interior; because *any* law of increase of temperature is compatible with the law that the increase is proportional to the depth near the surface. For if we represent the law of temperature by a curve starting from the surface whose axis of abscissæ is vertical, this merely amounts to saying that any number of different curves may be drawn which have the same tangent at the origin. But the law of increase of temperature below that outer part or crust of the earth, which we know to be solid, will be governed by the laws of convection if the interior be fluid, modified by change in the value of gravity, and by those of conduction if it be solid. Hence if there be a change of state from fluid to solid, there will necessarily be a break in the law at the depth where that occurs; and it will then be further complicated by the thermal changes which accompany the change of state.

Now the question as to whether the interior of the earth is at the present time solid or fluid, or partly solid and partly fluid,

[1] Pratt's "Figure of the Earth," 4th ed. Arts. 115—118.

may be attacked in two ways. The first of these is by considering what is the difference of effect that bodies exterior to it, namely the sun and moon, would have upon the motions of the earth in either case, and secondly by considering the sequence of events according to which a molten globe, such as the earth once was, may have passed into its present state.

For the first, Mr Hopkins, in the Phil. Trans. for 1839-40-42, investigated the consequences which, according to certain assumptions, would have resulted from the effect of internal fluidity upon the phenomenon of precession of the equinoxes, and he came to the conclusion that the solid crust of the earth could not be less than from 800 to 1000 miles thick.

His argument was assailed by Delaunay[1], but strongly supported by Sir Wm. Thomson and Archdeacon Pratt: while General Barnard[2] believed that Hopkins' result was vitiated by an oversight. It is however scarcely worth while to consider this argument further, because Sir Wm. Thomson has himself given up this particular reason against the doctrine of a fluid interior. "Interesting in a dynamical point of view as Hopkins' problem is, it cannot afford a decisive argument against the earth's interior liquidity[3]." To this change of opinion he was led by a conversation with Prof. Newcomb in America.

But there is another manner in which the sun and moon affect terrestrial motion; namely by raising the ocean tides. If the earth were not exceedingly rigid the globe itself would be raised in tides as well as the water which covers it, and since the measurable tide is the distance between the floor of the ocean and the surface of the sea, it is plain that if the floor of the ocean was drawn up along with the surface of the sea, the tide would be thereby diminished. Sir Wm. Thomson says: "The solid crust would yield so freely to the deforming influence of the sun and moon, that it would simply carry the waters of the ocean up and down with it, and there would be no sensible tidal rise and fall of water relatively to land[4]." Mr George Darwin has

1 "Geol. Mag." Vol. v. p. 507.

2 "Problems of Rotary Motion." "Smithsonian Contributions," No. 240. New Addendum, p. 42. Vol. XIX. 1871.

3 Sectional Address to British Association, 1876. 4 *Ibid.*

confirmed this conclusion. It does not however appear necessary that the earth should be absolutely solid from the surface to the centre in order to satisfy these requirements. May not the argument for rigidity drawn from the tides, after it has received that definite weight which proper observations (yet to be made) are expected to give it, be satisfied with a rigid nucleus of radius nearly approaching to that of the entire globe? Such tides as would be formed within the liquid substratum of the crust would not be of the nature of the tides, contemplated by Sir Wm. Thomson as affecting the entire spheroid, but more nearly analogous to the ocean tides; since they would involve a horizontal transference of fluid backwards and forwards, and might be expected to be of small amplitude, owing to the viscosity of the substance and its confinement beneath the crust. However, even the modest requirement of a fluid substratum is refused. "But now thrice to slay the slain. Suppose the earth this moment to be a thin crust of rock or metal resting on liquid matter. Its equilibrium would be unstable! And what of the upheavals and subsidences? They would be strikingly analogous to those of a ship that had been rammed; one portion of crust up and another down, and then all down. I may say with almost perfect certainty that whatever may be the relative densities of rock, solid and melted, at or about the temperature of liquefaction, it is, I think, quite certain that cold solid rock is denser than hot melted rock; and no possible degree of rigidity in the crust could prevent it from breaking in pieces and sinking wholly below the liquid lava[1]."

However, in a note to an address before the Geological Society of Glasgow, 14 Feb. 1878, Sir W. Thomson wrote, "Since this address was delivered, some important experiments have been carried out, at the request of Dr Henry Muirhead, by Mr Joseph Whitley of Leeds. His experiments were made on iron, copper, and brass, and on whinstone and granite; and the general result hitherto arrived at seems to be, that these substances are *less* dense in the solid than in the liquid state at the melting temperature[2]." And D. Forbes stated that glass

[1] Sectional Address to the British Association, 1876.

[2] "Trans. Geol. Society of Glasgow," Vol. VI. Pt. 1, p. 40.

floats on melted glass, and similarly Bessemer steel on melted steel, and says that Messrs Chance found the castings made from fused Rowley Rag were of the same size precisely as the wooden pattern. It is right to state however that the sand moulds were made red-hot to allow of slow cooling and devitrification[1].

If we now turn to Mr Hopkins' "Researches in Physical Geology[2]," we find that he had gone into this question somewhat fully, and held an exactly opposite opinion to the above. He considered that, so long as the matter of the earth retained a sufficiently high state of fluidity to admit of the circulation of convection currents, no crust could form: but that when, by those means, the temperature had been so far reduced that the currents became arrested, immediately a crust would be formed. "Since the heat increases with the distance from the surface, while the mass is cooling by circulation, the tendency to solidification, so far as it depends on this cause, will be greatest at the surface, and least at the centre. But on the other hand, the pressure is least at the surface, and greatest at the centre; and consequently the tendency to solidify, as depending on this cause, will be greatest at the centre, and least at the surface[3]." For want of experimental evidence, "the only conclusion at which we can arrive is this, that if the augmentation of the temperature with that of the depth be so rapid that its effect in resisting the tendency to solidify be greater than that of the increase of pressure to promote it, there will be the greatest tendency to become *imperfectly fluid*, and afterwards to solidify, in the superficial portions of the mass: whereas, if the effect of the augmentation of pressure predominate over that of the temperature, this transition from perfect to imperfect fluidity, and subsequent solidity, will commence at the centre[4]."

Assuming the latter to be the case, when the mass should have arrived at that stage of cooling that "a solid nucleus had been formed, surrounded by an external portion of which the

[1] "Chemical News," Vol. XVIII. p. 191.

[2] "Phil. Trans. Roy. Soc." Part II. 1839, quoted in his "Report to the British Association," 1847, p. 33.

[3] *Ibid.* [4] *Ibid.*

fluidity would vary continuously from the solidity of the nucleus to the fluidity of the surface, where, at the instant we are speaking of, it would be just such as not to admit of circulation;... a change would take place in the process of solidification which it is important to remark. The superficial parts of the mass must in all cases cool the most rapidly, and now (in consequence of the imperfect fluidity) being no longer able to descend, a *crust* will be formed on the surface, from which the process of solidification will proceed far more rapidly downwards, than upwards on the solid nucleus." And in a note to the Report[1], he writes thus decidedly: "Supposing the earth once to have been fluid, it must be now, or have been at some antecedent epoch, in that state in which a solid exterior rests on an imperfectly fluid and incandescent mass beneath. It is important to know that this state of the earth, assuming its original fluidity, is one through which it must necessarily have passed in the course of its refrigeration, whatever might be the process of its solidification."

These remarks of Mr Hopkins deserve serious consideration, and are not lightly to be set aside. They are entirely independent of his subsequent calculations by which he considered he had proved (and was until very lately considered by the greatest mathematicians to have proved)[2] that this crust is not at the present time a thin one, but has grown to the thickness of at least not far short of a thousand miles; even if its downward growth has not already met the upward growth of the solid central nucleus.

The reasoning by which Mr Hopkins concluded that a crust would be formed (and he clearly supposed it would be supported also), seems to be assailable only on the supposition that upon solidification a sudden considerable contraction, and consequent increase of density, would occur, which would enable the fragments to sink in a fluid, too viscous to admit of the sinking of portions cooled to the *verge* of solidification. But there is no reason whatever to suspect such a sudden considerable contraction. Indeed the experiments above referred to, point in a contrary direction.

[1] Page 48. [2] Vide supra, p. 22.

It is practically seen that a crust can be formed and supported upon liquid lava by what is known to happen in the crater of Kilauea. This is of an elliptical form, three miles in its longer diameter: consequently the attachment to the cliff-like sides can have little effect in supporting the crust over so large an area. Moreover, this has been observed to rise and sink with variations in the level of the lava, showing that it really rests upon the fluid support. "The floor of the pit is described as usually presenting a crust over a vast pool of lava, which is from time to time broken through by a fresh upboiling of the incandescent mass beneath. It cools and hardens so rapidly on exposure, that it may be walked upon within a few hours after its coagulating. Sometimes upwards of fifty small cones and craters, more or less in activity, have been counted on the floor of this great pit. They were from fifty to a hundred feet high[1]." "It is nine miles in circumference, and its lower area, which not long ago fell about 300 feet, just as ice on a pond falls when the water below it is withdrawn, covers six square miles[2]."

The conclusion which we may draw from these premises is that the earth is on the whole extremely rigid, but that from anything which we have as yet referred to, we cannot decide whether it is solid from the surface to the centre, or whether there may be a liquid substratum of fused rock, of no great depth perhaps, intervening between the crust solid from cold, and the nucleus, solid in spite of its high temperature from pressure. That such solidity is the case with the nucleus, appears necessarily to be the result of pressure, without reference to experiments regarding the contraction or expansion of rocks upon solidifying, for otherwise the proved rigidity of the earth would not be compatible with a high temperature—unless indeed, as suggested by Sir Wm. Thomson, the nucleus be a cool sphere, around which a stratum of meteoric matter has accumulated, heated to the temperature of fusion by its collision with the pre-existing nucleus. This latter

[1] Scrope's "Volcanos," p. 477, ed. 1872.

[2] Miss Bird's "Hawaiian Archipelago." See "Nature," Vol. XI. p. 324.

supposition however may be passed over as not affecting the aspect of the subject at present under review.

It is possible that a future generation of geologists may be able to gather some information regarding the interior of the earth from the mysterious phenomena of magnetic variation. But at present we cannot hope for much help from that quarter. The temptation, however, is too great to forbear quoting from Captain Evans a very striking passage[1]. "These are a few facts relating to secular changes going on in two magnetic elements within our own time; and what are the inferences to be drawn therefrom? They appear to me to lead to the conclusion that movements, certainly beyond our present conception, are going on in the interior of the earth; and that so far as the evidence presents itself, secular changes are due to these movements, and not to external causes. We are thus led back to Halley's conception of an internal nucleus or inner globe, itself a magnet, rotating within the outer magnetised shell of the earth." This is, to say the least, in very remarkable accordance with the conclusion that a liquid stratum underlies the cooled crust of the earth.

[1] Lecture at the Royal Geographical Society, March 11, by Capt. F. J. Evans, C.B., F.R.S., Hydrographer to the Admiralty, "Nature," May 16, 1878.

CHAPTER III.

INTERNAL DENSITIES AND PRESSURES.

Speculations concerning the materials in successive couches—Waltershausen's theory—His law of density open to correction—His tables of densities and pressures—Fluid pressures within the earth calculated from Laplace's law of density—Fluid pressure at the centre.

THE law of density in the interior of the earth has naturally led to speculations concerning the composition of the successive couches, or shells, of which it is composed. Waltershausen, in his "Rocks of Sicily and Iceland," has formed a theory of the earth on this basis. It is thus epitomised by Mr Clarence King in the "Geological exploration of the fortieth parallel," Vol. I. p. 710[1]: "To sum up the theory of Waltershausen; the earth is a hot globe, of which a considerable portion is fluid, an unknown fraction of the centre having been rendered solid by the raising of its fusion-temperature by pressure. The downward increment of density is expressed by the chemical increment of the heavy bases, and the fluid region directly under the crust consists, first, of a feldspathic and acidic magma which passes downwards by successive replacements of bases into an augitic, and finally into a magnetitic magma."

The formula used by Waltershausen[2] for the calculation of the density ρ' at a distance r from the centre is of the form

$$\rho' = \mathrm{P} - (\mathrm{P} - \rho)\, r^2 :$$

[1] Washington, Government printing office, 1878.

[2] "Rocks of Sicily and Iceland," p. 315.

where ρ is the surface density, and P the density at the centre. The radius of the earth is here taken as unity. In order to render the equation homogeneous we may write it, a being the earth's radius,

$$(P - \rho')\, a^2 = (P - \rho)\, r^2,$$

or

$$\frac{P - \rho'}{P - \rho} = \frac{r^2}{a^2}.$$

Now Laplace's equation, which leads to the law of density already referred to[1], is

$$\rho' = Q \frac{\sin qr}{r},$$

where Q and q are constants to be determined. Consequently the central density will be given when

$$r = 0,$$

or

$$P = Q \frac{\sin qr}{qr} q \quad \text{when } r = 0,$$

$$= Qq.$$

Also the surface density will be given by putting

$$r = a;$$

$$\therefore \ \rho = Q \frac{\sin qa}{a};$$

$$\therefore \quad \frac{P - \rho'}{P - \rho} = \frac{q - \dfrac{\sin qr}{r}}{q - \dfrac{\sin qa}{a}} = \frac{r^2}{a^2} \frac{\dfrac{qr - \sin qr}{r^3}}{\dfrac{qa - \sin qa}{a^3}}.$$

This is the correct expression for the ratio $\dfrac{P - \rho'}{P - \rho}$. And consequently Waltershausen's value of that ratio $\left(\text{viz. } \dfrac{r^2}{a^2}\right)$ is strictly true only when the relation is satisfied,

$$\frac{qr - \sin qr}{r^3} = \frac{qa - \sin qa}{a^3}.$$

When the constants Q and q are determined in accordance with the mean values above given of the density of the whole

[1] p. 20.

earth and of the surface density, it is found that

$$qa = 2{\cdot}4605 = 140^\circ\ 58'\ 35'';$$

and $$Qq, \text{ or } P = 10{\cdot}74,$$[1]

which is therefore the density at the centre of the earth.

In order to see to what extent Waltershausen's formula is incorrect let us apply it to the instance where

$$r = \frac{a}{2}.$$

We find that the value of $\dfrac{P-\rho'}{P-\rho}$ for this value of r, according to Laplace's formula, is

$$\tfrac{1}{4} \times 1{\cdot}2566:$$

whereas Waltershausen's formula gives

$$\frac{P-\rho'}{P-\rho} = \frac{1}{4}.$$

Consequently it gives too small a value for this ratio.

DENSITIES ACCORDING TO WALTERSHAUSEN.

RADIUS.	DENSITY.	
1·00	2·66	
0·99	2·79	
0·98	2·93	
0·97	3·07	Lime.
0·96	3·20	Magnesia.
0·95	3·34	
0·94	3·47	
0·93	3·60	
0·92	3·72	
0·91	3·85	
0·90	3·99	Alumina.
0·80	5·15	Iodine, Iron oxide.
0·70	6·29	Tellurium, Chromium.
0·60	7·09	Zinc, Iron, Tin.
0·50	7·85	Cobalt, Steel.
0·40	8·47	Uranium, Nickel.
0·30	8·96	Copper.
0·20	9·31	
0·10	9·51	
0·00	9·59	Bismuth, Silver.

[1] Pratt's "Figure of the Earth," 4th ed. Art. 115.

PRESSURES ACCORDING TO WALTERSHAUSEN.

RADIUS.	ATMOSPHERES.
1·00	0
0·99	17138
0·98	34591
0·97	53070
0·96	72195
0·95	92432
0·94	113180
0·93	134600
0·92	156840
0·91	179680
0·9	203320
0·8	471680
0·7	786080
0·6	1125690
0·5	1468000
0·4	1701500
0·3	2297500
0·1	2441900
0·0	2492600

It is an interesting enquiry what the fluid pressure would be according to Laplace's law of density now assumed. For the actual pressure is probably not very different from the fluid pressure; because, if the rigidity of the interior be due to pressure, it will have been fluid at any given level until the temperature had fallen sufficiently for the pressure to overcome the fluidity, so that the pressure there will be the fluid pressure. But even if that be not a true account of the process of solidification, still it is not probable that the couches can be appreciably supported in the manner of an arch; in which case we may consider the pressure at any point in a prism from the surface to the centre to be due to the weight of the portion of the prism above the level under consideration[1].

Let p be the pressure at the distance r from the centre of the sphere, ρ' the density and g' the gravity at that level.

Then as r is increased p is diminished, and we shall have

$$\frac{dp}{dr} = -g'\rho' \quad \text{..........................(1)}.$$

[1] At Dukinfield colliery at 2500 ft. circular arches of brick, probably five to six feet in diameter, and four feet thick, were crushed. Hull's "Coal Fields of Great Britain," p. 447, quoted in "Jour. of Science," No. xxxix.

Since the attraction of the portion of the sphere exterior to the shell, whose radius is r, upon a particle within it is nothing, the value of g', due to the remaining portion, will be,

$$g' = \frac{\int_0^r 4\pi r^2 dr \rho'}{r^2}.$$

Substituting for ρ' its value $\frac{Q \sin qr}{r}$ and integrating, we have

$$g' = \frac{4\pi Q}{q^2 r^2}(\sin qr - qr \cos qr),$$

and therefore,

$$g'\rho' = \frac{4\pi Q^2}{q^2 r^3}(\sin^2 qr - qr \sin qr \cos qr).$$

Hence, g and ρ being the values of g' and ρ' at the surface,

$$\frac{g'\rho'}{g\rho} = \frac{a^3}{r^3}\frac{\sin^2 qr - qr \sin qr \cos qr}{\sin^2 qa - qa \sin qa \cos qa},$$

$$= A\frac{\sin^2 qr - qr \sin qr \cos qr}{r^3}.$$

Substituting for $g'\rho'$ from (1), and integrating by parts,

$$\frac{p}{g\rho} = \frac{A}{2}\frac{\sin^2 qr}{r^2} + C,$$

when $$r = a, \ p = 0;$$

$$\therefore \quad p = \frac{g\rho A}{2}\left(\frac{\sin^2 qr}{r^2} - \frac{\sin^2 qa}{a^2}\right).$$

If we write na for r, and restore the value of A, this may be put in the form,

$$p = \frac{g\rho a}{\sin 2qa\,(\tan qa - qa)} \times \left(\frac{\sin^2 qna}{q^2 n^2 a^2} q^2 a^2 - \sin^2 qa\right).$$

This gives the pressure at any depth, the only variable quantity in the expression being

$$\frac{\sin^2 qna}{q^2 n^2 a^2},$$

which at the centre, where $n=0$, becomes unity. Hence the pressure at the centre is

$$\frac{g\rho a}{\sin 2qa\,(\tan qa - qa)}(qa + \sin qa)(qa - \sin qa).$$

To calculate the pressure in pounds on the square foot, we observe that $g\rho$ is the weight of a cubic foot of rock at the surface, or

$$g\rho = 2{\cdot}75 \times \text{weight of a cubic foot of water};$$
$$= 171\ 875 \text{ pounds.}$$

Archdeacon Pratt has found for the mean values of the axes of the spheroid corrected for local attraction[1],

$$\alpha = 20926184 \text{ feet,}$$
$$\beta = 20855304 \text{ feet.}$$

If we define the mean sphere as one of equal volume with the spheroid, then

$$a^3 = \alpha^2\beta,$$

and $$\therefore\ a = 20{,}902{,}520 \text{ feet};$$

which we will take to be the value of the mean radius.

Then, with the values of qa already given[2], we obtain for the pressure upon a square foot at the earth's centre

6,351,810,000 pounds.

Reducing this to atmospheres at 14·7 pounds per square inch, the fluid pressure at the centre of the earth will be found to be

3,000,660 atmospheres.

This, as will be observed, considerably exceeds Waltershausen's estimate[3].

[1] Pratt's "Figure of Earth," 4th ed. Art. 173.
[2] p. 30.
[3] See Table, p. 31.

CHAPTER IV.

LATERAL PRESSURE, ITS AMOUNT.

Generally received views regarding the contortion of rocks by lateral pressure—Calculation of the possible amount of such pressure at the surface—Attempt to estimate the same within the earth—Mountain chains elevated by lateral pressure—The Himalayas.

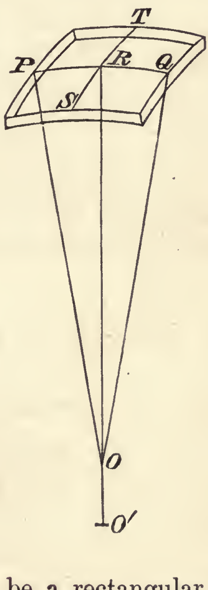
THE well-known fact that great lateral compression has affected the stratified rocks of the earth's crust, not once, but again and again from the earliest to the latest of the extended periods of Geological time, is now generally explained by the supposition that the globe has contracted through secular cooling[1]. *Without enquiring whether the whole globe did or did not become solid at the same time*, we may admit that when the outer crust became solid it must have assumed very nearly the temperature of the atmosphere, owing to its heat being radiated away into space. It is thought that as the cooling proceeded, the interior shrunk away from the crust, and the latter became wrinkled; and that it was by this means that the crumpling and contortions of the rocks were produced. The simile sometimes made use of in Geological text-books is that of the skin of a shrivelled apple. Where these wrinkles have not yet been

[1] The earliest publication of this theory appears to have been in a foot-note to a memoir on the mountains of L'Oisans ("Mémoires de la Société d'Histoire Naturelle," t. v. 1829) by M. Elie de Beaumont. The passage is quoted by Prof. A. de Lapparent, in a paper entitled "L'origine des inequalities de la surface du globe," in the "Revue des Questions Scientific," Juillet, 1880.

Prof. Sedgwick proposed the same theory two years later, in his paper on the structure of the Cumbrian mountains, "Trans. Geol. Soc.," Jan. 5, 1831. It occurred also to the Author, as he believes independently, about 1841, and was suggested to him by the wrinkles formed on old ice, when the surface of the water had sunk away from it. He has however been led to change his opinion concerning the cause of contraction, as will appear further on.

denuded and levelled down, they now form mountain chains, in which the crumpling of the rocks is usually conspicuous. In other cases the tracts so elevated have been planed down by meteoric and marine action, afterwards covered by horizontal deposits, and re-elevated with greater or less secondary crumpling, into the positions where we now see them. That enormous force has been concerned in this crumpling of the hardest rock is obvious. Let us estimate its amount; premising that we make no assumption whatever regarding the *condition* of the interior of the globe nor the cause of contraction, but merely assume that it has shrunk away from a rigid upper crust. The problem is mathematically analogous to that of finding the tension of a flexible surface exposed to the pressure of a fluid.

The form of the earth approaches very nearly to that of an oblate spheroid of revolution whose equatorial semiaxis (a) is 20926184 feet and whose polar semiaxis (b) is 20855304 feet[1]. Its ellipticity as already stated is very nearly $\frac{1}{295}$.



Suppose $PTQS$ to be a rectangular element of the crust, the four sides being equal and the thickness being τ, and let

[1] p. 33.

PQ and ST be the curves of greatest and least curvature on its surface. These are by a theorem in geometry necessarily orthogonal. Let RO, RO', the corresponding radii of curvature, be called r, r'.

Let T be the pressure on unit of area of the section of the crust in the directions of the tangents at P and Q which is produced by the lateral thrust. Similarly let T' be the pressure in the direction of the tangents at S and T. The weight of the element is $g\rho\, PQ \times ST \times \tau$, and acts in the normal ROO'.

Let the angles
$$POQ = \theta,$$
$$SO'T = \phi.$$

If then we suppose this element supported by a pressure R on unit of area from beneath, resolving the forces vertically we have for the equation of equilibrium

$$2Tr'\phi\tau \sin\frac{\theta}{2} + 2T'r\theta\tau \sin\frac{\phi}{2} = g\rho r\theta r'\phi\tau - Rr\theta r'\phi.$$

In the limit, dividing by $r\theta r'\phi$ this gives

$$\left(\frac{T}{r} + \frac{T'}{r'}\right)\tau = g\rho\tau - R.$$

If we regard the surface as spherical and of radius a, T and T' become equal, and we have

$$2\frac{T\tau}{a} = g\rho\tau - R.$$

If the support beneath is withdrawn altogether R becomes nothing and then

$$T = \tfrac{1}{2} g\rho a,$$

which gives the maximum thrust which would be produced by the shrinking away of the underlying mass.

The meaning of this result is that the horizontal force of compression thrown into a stratum at the earth's surface by the shrinking away of the underlying parts would be equal to the weight of a piece of the same stratum of the same section as the stratum and *two thousand* miles long—enough to crumple up and distort any rocks. It would be about 830200 tons upon the square foot[1].

[1] Calculated from a cube of granite weighing 178·339 lbs.

The pressures T and T' being orthogonal cannot affect one another. Suppose then T and r given. It follows that if r' is increased or diminished T' must be increased or diminished in the same proportion. Hence, if the curvature be diminished the pressure will be proportionately increased and *vice versâ*. Hence there will be no greater tendency to compression where the curvature is greater, as about the equator, than where it is less, as about the poles, but the reverse. This answers an objection which has been sometimes brought against the theory; although apart from the above reasoning it is scarcely likely that so small a difference of curvature as that referred to can have any appreciable consequence.

It is inconceivable that such rock as we find at the surface of the earth could support so great a pressure as this without bending and breaking. Hence, if it be supposed possible that for a moment a shell of moderate thickness, say a few miles, should be self-supporting, and therefore exert no pressure upon that immediately beneath it, this state of things could not continue, and the outer shell would give way, and settle down upon the next stratum below it.

We are not able however to estimate the downward pressure which will be exerted by it at any point upon the next lower stratum, because the lateral pressure T might not be wholly relieved, but only partially so, and so to some extent help to support the shell.

It is evident that in such a case the downward pressure would not be the same everywhere, the tilted rocks in some places supporting each other, and pressing with diminished weight upon the subjacent matter.

If the rock were perfectly rigid T would have its full value, the shell would be self-supporting, and the vertical pressure on the next stratum would be nothing. If on the other hand the rock were fluid, such pressure would be that due to the depth τ of the upper layer. Its actual value will be something intermediate.

We will next make an attempt to estimate the lateral pressure at points within the earth, upon the supposition that the interior is solid, in order to see whether any stratum would be

likely to be crushed under the circumstance of a contraction of the mass beneath.

Suppose that, on a unit of area, P is the pressure towards the centre at a distance r from the centre, caused by so much of the weight of the superincumbent rock as is sustained by the next inferior stratum (which is supposed not to be yet crushed). Then, if we consider the effects of the crushing of the rock above it to be uniformly distributed, P cannot be so great as the pressure which would arise from the same rock were it fluid, because part of the weight of the crushed rock will be sustained by lateral pressure.

Hence if we consider the conditions of equilibrium of an element of the stratum in question lying next beneath the rock which produces the pressure P, and suppose this shell and the superincumbent mass to be in equilibrium without resting upon the interior mass, we shall be able to calculate the horizontal pressure which tends to crush the shell.

Let T' be the horizontal pressure upon a unit of area of any vertical section of this shell, τ' the thickness of the shell, and g', ρ', the values of g, ρ, at that depth.

Then we shall have, by the same reasoning as in the previous proposition,

$$T'\tau' = (P + g'\rho'\tau')\frac{r}{2}.$$

Let us compare this horizontal pressure T' with the corresponding pressure T at the surface, which has been shown to be equal to $g\rho\frac{a}{2}$,

$$T' >=< T.$$

As $$\frac{P + g'\rho'\tau'}{\tau'}\frac{r}{2} >=< \frac{g\rho a}{2}.$$

As $$Pr >=< (g\rho a - g'r'r)\tau'.$$

As $$P >=< g\rho\left(\frac{a}{r}\tau' - \frac{g'\rho'}{g\rho}\tau'\right).$$

Let y be the depth of a column of rock of a unit of section and density ρ, whose weight at the surface would equal the

pressure $g\rho\left(\frac{a}{r}\tau'-\frac{g'\rho'}{g\rho}\tau'\right)$ upon a unit of surface, with which we have now to compare P.

Then $$g\rho y = g\rho\left(\frac{a}{r}\tau'-\frac{g'\rho'}{g\rho}\tau'\right),$$

and $$y=\frac{a}{r}\tau'-\frac{g'\rho'}{g\rho}\tau' \text{ ....(1).}$$

In order to express the right-hand side in terms of r, we must assume a law of density and calculate the value of $\frac{g'\rho'}{g\rho}$. This has been already done (p. 32), and it has been found that

$$\frac{g'\rho'}{g\rho}=\frac{a^3}{r^3}\frac{\sin^2 qr - qr\sin qr\cos qr}{\sin^2 qa - qa\sin qa\cos qa}.$$

Now

$$\sin^2 qr - qr\sin qr\cos qr = \sin^2 qr\left(1-\frac{qr}{\tan qr}\right).$$

Let us next divide the earth as Archdeacon Pratt has done[1] into four shells and a central nucleus, the radius of the nucleus and the thickness of each shell being equal to one-fifth of the earth's radius.

The corresponding values of qr and of $\frac{qr}{\tan qr}$ are there given, viz. commencing at the surface,

$$\begin{aligned} & \qquad\quad\text{in arc} \\ qa &= 2{\cdot}4605 = 140^\circ\ 58'\ 35'', \\ q\frac{4a}{5} &= 1{\cdot}9684 = 112^\circ\ 46'\ 52'', \\ q\frac{3a}{5} &= 1{\cdot}4763 = 84^\circ\ 35'\ \ 9'', \\ q\frac{2a}{5} &= 0{\cdot}9842 = 56^\circ\ 23'\ 26'', \\ q\frac{a}{5} &= 0{\cdot}4921 = 28^\circ\ 11'\ 43'', \\ \frac{qa}{\tan qa} &= -3{\cdot}035906. \end{aligned}$$

[1] Art. 119, 4th ed.

And the other values of $\frac{qr}{\tan qr}$ are for the several shells,

$$-0{\cdot}826756,$$
$$+0{\cdot}139920,$$
$$+0{\cdot}654135,$$
$$+0{\cdot}917944.$$

Hence calculating the value of

$$\sin^2 qa - qa \sin qa \cos qa,$$

equation (1) becomes

$$y = \frac{a}{r}\tau' - 0{\cdot}62499 \frac{a^3}{r^3} \sin^2 qr \left(1 - \frac{qr}{\tan qr}\right)\tau'.$$

It is easily seen that $\sin^2 qr \left(1 - \frac{qr}{\tan qr}\right)$ is of four dimensions in r, so that the second term vanishes when $r = 0$.

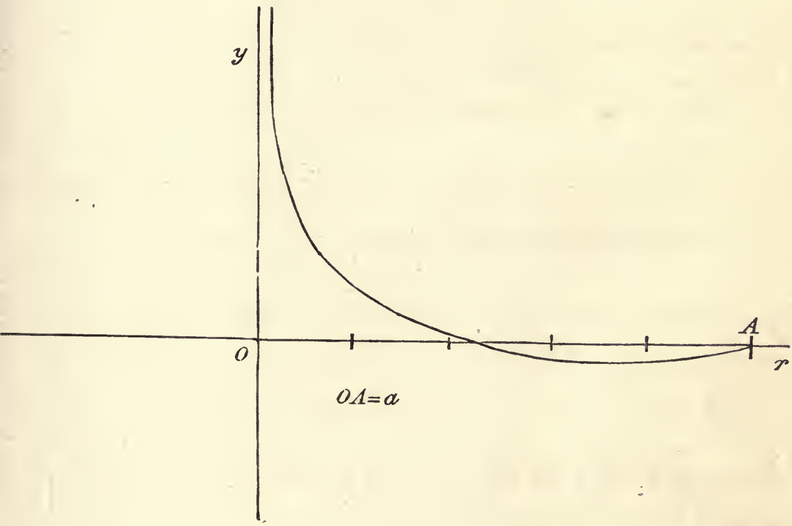
Moreover, between the values of $qr = qa$ and $qr = \frac{\pi}{2}$, $\frac{qr}{\tan qr}$ increases from $-3{\cdot}035906$ to 0, and continues to increase from 0 to 1 when $qr = 0$; so that between those limits $1 - \frac{qr}{\tan qr}$ decreases from $4{\cdot}035906$ to 0. It appears then that $\frac{g'\rho'}{g\rho}$ decreases from 1 at the surface to 0 at the centre.

And if we give to r the values

$r = a$,	then	$y = 0$.
$r = \frac{4}{5}a$,		$y = -0{\cdot}6455\tau'$.
$r = \frac{3}{5}a$,		$y = -0{\cdot}7997\tau'$.
$r = \frac{2}{5}a$,		$y = 0{\cdot}1573\tau'$.
$r = \frac{1}{5}a$,		$y = 3{\cdot}5689\tau'$.
$r = 0$,		$y = \infty$.

So that the curve representing the values of y from $r = 0$ to $r = a$ will be of the subjoined form, having the axis of y for an asymptote.

The reason why the ordinate of this curve contains τ', is because it is obtained from the conditions of equilibrium of an element of an internal shell supporting, besides its own weight, part of that of the rock above it, so that, the thicker the shell, the less will be the lateral pressure on a unit of vertical section; because the vertical pressure is not proportional to the thickness of the shell, as it is in the case first considered of a shell at the surface of the earth.



It must be borne in mind that, in the two cases which have been discussed, τ and τ' must be taken so small, that gravity and density may be considered constant through those thicknesses.

Resuming;

It has been shown that the horizontal pressure T' on a unit of surface of a vertical section of a shell of thickness τ' at a distance r from the centre will be $> = <$ than the corresponding pressure at the surface,

as the downward pressure P at that depth

$$>=<g\rho\left(\frac{a}{r}\tau'-\frac{g'\rho'}{g\rho}\tau'\right)$$

$>=<g\rho\times$ the height of a column of rock represented by the ordinate of the curve.

Represent P by the weight of a column of rock at the surface of height equal to z.

Then $P=g\rho z$; where g, ρ are the values at the surface, and are constant.

And $z=f(r)$ will be some curve whose ordinate will bear the same ratio to P which that of the above curve (1) does to the pressure with which we have to compare P.

Now all we know about this curve $z=f(r)$ is from the nature of the case, which shows that its ordinate is always positive, that it is zero when $r=a$, and does not become infinite between $r=0$ and $r=a$.

Hence, comparing the two curves, they will have a point in common when $r=a$, $y=0$; and the ordinate of the latter (which represents P) will be greater than that of the former (it being negative) until they intersect, which they must do somewhere between $r=0$ and $r=\frac{2}{5}a$.

Hence, the inequality shows that the lateral pressure within the earth becomes equal to that at the surface when $r=a$ (as it ought to do): while for values of r less than a it is always greater than that at the surface, until some point is reached, which cannot be further from the centre than $\frac{2}{5}$ of the radius, and is probably much nearer to it. There the curves will intersect and the lateral pressure within the earth will be equal to that at the surface. Afterwards it becomes smaller and eventually infinitely smaller.

The inference from this is that no shell is likely to have sufficient strength to resist the crushing effects of lateral pressure unless it be at a very small distance from the centre. And hence the supposition made by some geologists that the internal strata will be able to sustain the superincumbent weight on account of their dome-shaped form seems on the whole untenable. The result of this will be that, even if the earth be solid, foldings and dislocations of the strata commencing at the surface, will be

propagated downwards as far as the causes which tend to produce them are felt.

Let us now return to the consideration of the external layers of rock, supposed to consist to the depth to which we are acquainted with them chiefly of sedimentary strata.

It has been shown that if an external layer were supposed for a moment to be unsupported from beneath, it would be acted upon horizontally in any two directions at right angles to each other, by pressures which, upon a given area of section, would be equal to the weight of a column of rock of the same sectional area, and of the length of half the earth's radius.

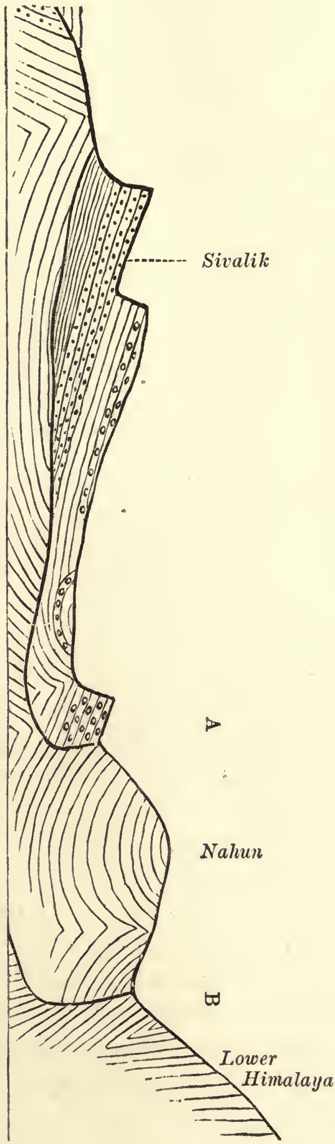
We may admit then that the crumpling by lateral pressure, which is such a common phenomenon among rocks of all ages, and especially among the older rocks, is due to the force, whose amount has here been approximately calculated. Wherever from any local weakness the superficial strata have been disposed to yield, they will have formed a ridge, or a series of ridges; and the pressures at any point being resolvable horizontally into two at right angles to one another, which can in no way neutralize each other, the chains of mountains which traverse a country, if of equable elevation, may *à priori* be expected to form a network, or if there be one great master chain, like the Andes, it may be expected to be supplemented by a number of smaller chains at right angles to it, the amount of relief which has been afforded by the great chain in one or two great folds, being equal to the sum of what has been afforded in the perpendicular direction by the lesser chains in many lesser folds. A semi-lunar arrangement of a chain would give relief in both directions at once. But whether the formation of a chain of mountains in this manner is compatible with a solid globe is doubtful.

To take an instance of the mode of action of lateral pressure we may refer to a very instructive comparison between the Alps and Himalayas which has been made by Mr Medlicott, and published in the Journal of the Geological Society[1]. He there shows that the reversed apparent faulting by which the older rocks of the central chain of the Himalayas appear to overlap

[1] "Jour. Geol. Soc.," Vol. XXIV. p. 34.

the younger of the Nahun range, and these again the still younger rocks of the Sivalik range, is due to lateral pressure, which must have compressed the rocks horizontally, at least at two distinct epochs since the central chain was first elevated.

An inspection of Mr Medlicott's diagram will show that the lateral pressure, which caused the overhanging surfaces of junction at *A* and *B*, acted with greater force in the lower than in the upper part of the section. This could not have been the case unless the chain had been already raised when compression last took place.



CHAPTER V.

ELEVATIONS AND DEPRESSIONS.

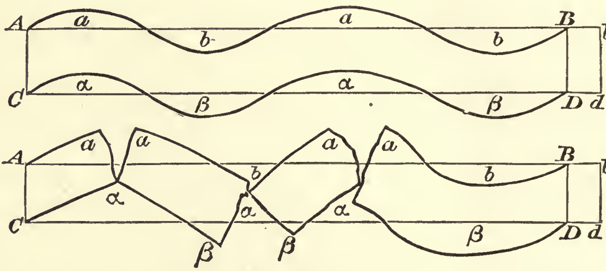
Distinction between compression and contraction—Definition of datum levels—General geometrical relation between the compression of a portion of the earth's crust and the elevations and depressions referred to the upper datum level resulting therefrom—The same extended to the whole surface of the globe—Modification of this relation if the globe is supposed to be wholly solid—Average height of all the elevations above the datum level upon the supposition of solidity, expressed in terms of depth of sea and height of land, and of areas of the same—This average height of actual elevations estimated in feet.

It was proved in the last chapter that the horizontal thrust, or force of compression, thrown into a stratum at the earth's surface, or into any stratum to a very considerable depth below the surface, by the shrinking away of the underlying parts would be abundantly sufficient to crush the strata so left unsupported; and it was explained how this crushing is supposed to have given rise to mountain chains.

The object of the present chapter is to find a relation between the quantity of the matter elevated and the amount of compression to which its elevation has been due. It is necessary to distinguish between the terms "compression" and "contraction." Contraction of the underlying rocks causes compression in the rocks above them. Of course contraction and compression may go on simultaneously in the same stratum. It may be compressed by the contraction of the rocks beneath it and it may contract by itself cooling, or by losing some portion of its constituent substance. But for the present we shall consider that the stratum which is compressed does not at the

same time contract, nor lose anything in volume by the compression to which it is subjected.

Let $ABCD$ be a layer of rock of unit of width, length l, and depth k. And suppose the abutments at AC and BD to approach each other through the space le, where e is a small



fraction, which we may call the coefficient of compression. Then the layer of rock in question would assume some new form, suppose one of those given in the figure, or any other whatsoever conceivable[1].

Let us now seek for some simple laws which must govern the disturbed strata in spite of the confusion which appears to reign among them. Let a, a, &c., be the areas formed by the upper curved line above AB, and b, b, &c., the areas formed by the same line below AB. It is not necessary that the a's should be equal to one another, nor yet the b's. They are used simply to designate the areas in respect of their *positions*. We will call AB "The datum level."

In like manner let α, β, be similar areas for the lower datum level CD. Then the space included between the curved lines must be equal to $ACdb = kl(1+e)$.

It is also evidently equal to

$$ACDB + a + a + \&c. + \beta + \beta + \&c.$$
$$- b - b - \&c. - \alpha - \alpha - \&c.,$$

[1] Some interesting experiments have been made by Prof. A. Favre of Geneva upon the effect of compression in raising elevations of a plastic substance. He placed a layer of clay upon a stretched band of Caoutchouc, and then allowed it to contract. A description of the experiments, and figures illustrating some of the results, which closely resemble the contortions sometimes seen in the earth's strata, will be found in "Nature," Vol. XIX. p. 103. See also Ch. X. seq.

or, denoting the sums of the quantities in like positions by the symbol, Σ, we get

$$kl(1+e) = kl + \Sigma(a) - \Sigma(b) + \Sigma(\beta) - \Sigma(\alpha).$$

$$\therefore kle = \Sigma(a) - \Sigma(b) + \Sigma(\beta) - \Sigma(\alpha) \qquad\qquad (1),$$

which we shall call a "datum-level equation."

Since the pressure is supposed to take place in a horizontal direction, it will not have any direct effect to raise the centre of gravity of the portion of the crust under consideration; so that, if the layer in question rest upon a liquid substratum, we may expect some portions of the disturbed crust to dip into the superheated rocks. But in that case, if the datum-level be unaffected in position, a corresponding volume of such subjacent rock must rise into the anticlinals.

Hence, $\Sigma(\alpha) = \Sigma(\beta)$.

And the equation becomes

$$kle = \Sigma(a) - \Sigma(b).$$

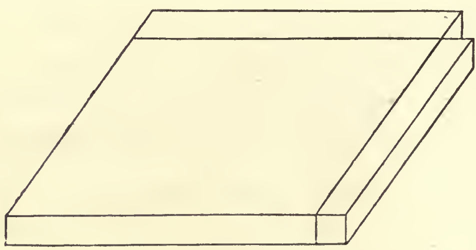
In order to render this reasoning applicable to the section of a surface of any form it is only necessary that the pressure, which causes the compression, should be everywhere tangential to the surface, and that gravity should be perpendicular to it. Hence it is applicable to the earth's surface, although that surface is not strictly regular, and may contain local elevations and depressions affecting the *mean* figure (that is the figure as unaffected by corrugation), which, though of small amount as compared to the dimensions of the earth, may be large as compared with the quantities of which we have to take cognizance in this investigation.

But the above suppositions cannot represent accurately what has occurred in nature. For they assume the upper and under parts of the crust of the earth to have been compressed horizontally by the same amount. In reality compression must have gone on gradually ever since a solid crust was first formed, and must be greatest at the upper datum level. We must therefore consider e to be the mean coefficient of compression. In the next place it will be necessary to extend our considera-

tions from a section of unit of horizontal width to any proposed *area* of the earth's surface, and eventually to that of the whole globe.

A clear conception of what will then be the upper "datum level" is important. It will be an imaginary surface which occupies the position that the surface of the crust would occupy at the present time, had it been perfectly compressible in a horizontal direction; so that no corrugations would have been formed in it. At present we regard it as affected in position by the contraction of the interior, but not by the compression of the crust.

As soon as elevated tracts had once been formed by the corrugation of the cooled crust, all the material above the datum level, however distributed, must have been derived from matter originally beneath it, and subsequently raised above it. As often as further compression of the crust has taken place, every fresh addition to the sum total of the quantity of matter above the datum level must have accrued by the elevation of matter from below it. For the matter which was already above it, however freshly corrugated, or re-arranged by water action, or otherwise, cannot have been thereby altered in quantity. Moreover each additional contraction will have acted upon a crust thicker than it was before, on account of its having become in the meanwhile solid, or if solid contracted, to a greater depth.



We have hitherto confined our symbols to a vertical section of unit of width. We will now extend them as in the diagram to a portion of the crust whose length is l and width w, and the depth k; e and e' being the mean coefficients of compression in the directions of length and width. Then, if we

neglect the product ee', our equation will become

$$klw(e+e') = \Sigma(A) - \Sigma(B),$$

where A and B are now the *volumes* of the elevations above and depressions below the datum level.

If in this equation we put $w = 1$ and $e' = 0$, it reduces to our former equation.

If we put $e = e'$ we get

$$2klwe = \Sigma(A) - \Sigma(B) \qquad (2),$$

which we may take as the general expression corresponding to any area of the surface.

The relation just found manifestly assumes that before any compression took place, that is at the epoch when the crust first solidified, there were neither elevations nor depressions upon its surface; or in other words it assumes that the surface was one of equilibrium, like that of the ocean. And if the crust passed from a liquid, through a viscous, to a solid condition, this seems to be the natural supposition. And this supposition is strengthened by the fact that all the rocks we see at the surface have either been water-deposited, or else have been protruded through water-deposited rocks. So that no evidence of any pristine inequalities can be adduced which is opposed to this natural supposition.

The tendency of the corrugations over a given area will be to form two systems at right angles to one another, thus relieving the whole compression. Where one corrugation intersects another, we shall have a part of the volume common to both. But physically the same space cannot be occupied by two distinct volumes of rock. Hence at every such intersection there must be an increase of altitude in the ridges sufficient to contain within the contour an additional volume equal to the common portion. This appears to occur in nature where a peak often occupies the place of intersection of two ranges, as if the denudation which has shaped out the mountains has had a greater amount of matter to remove in such a situation.

The result expressed by the equation (2) may be extended to

the whole globe by considering its surface composed of elementary rectangular areas, and summing them; whence

$$8\pi r^2 ke = \Sigma(A) - \Sigma(B) \ldots\ldots\ldots\ldots\ldots\ldots (3).$$

In which expression

r is the radius of the earth measured to the present position of the datum level.

k is the present thickness of the cooled crust.

e is the *mean* total linear compression of the crust.

$\Sigma(A)$ is the volume of the total elevations above the datum level.

$\Sigma(B)$ is the volume of the total depressions below it.

And $\pi = 3{\cdot}14159$.

It is worthy of remark, that the relation between lateral compression and elevation expressed by the above equation, (3), in no way depends upon the arrangement of the disturbed rocks, nor upon the time at which the successive movements have taken place, nor upon the elevations occupying their original positions on the globe, nor upon whether or not subsequent denudation, or any other mode of action, has redistributed the elevated matter. Nay, even if by human agency excavations have been made, their results will be included in our equation, which is perfectly general and true, so long as it is strictly interpreted. It requires also in particular to be noticed that it is not assumed that mountain-ranges need be anticlinals. All that is assumed is that they are carved out of tracts elevated as a whole by lateral pressure.

An opinion seems to have lately gained ground among geologists, that the continents and oceans have at all periods occupied nearly the same positions upon the globe that they do now. It may be as well to remark that, whether this opinion be true or not, it has no effect whatsoever upon the correctness of the result at which we have arrived. Our equation does not depend in the least degree upon the intermediate history of the earth's surface; but requires only that in the primeval state it

should have been a level surface, and that it is at present what we see it to be.

Another point to which the reader's attention is requested is that no assumption whatever has been hitherto made either as to the amount of contraction or of compression, except that the latter is small compared to the dimensions of the area compressed. The rude diagrams, which have been used in proving our equation, are not supposed to represent any thing in nature.

This appears to be the proper place for considering the manner in which the results of the deposition of sediment over a limited area would enter our equation. Geologists believe thick deposits to have been accumulated upon sinking areas, and attribute the subsidence of the area to the weight of the deposit itself. If rocks, liquid or plastic from heat or any other cause, come within a moderate distance of the surface this is possible, and many instances both among ancient strata and modern river deposits seem to be explicable on no other hypothesis. But the displacement of such plastic matter from beneath the new deposit must cause the rise of an equal volume somewhere else, so that if the terms of our equation were in any way affected (which would depend upon whether the effect extended below the datum level) it would be by an addition to the term $\Sigma(\beta)$, and a consequent equal addition to $\Sigma(\alpha)$, and their sum would be zero. So that, as already intimated, our equation will include any such effect of denudation and the resulting deposition.

Such an action as has been just referred to would produce a slight amount of crumpling in the strata, but only a very slight one, unless the new deposit *sunk through* some subjacent strata, and pressed them sideways out of its way; as the Author has shown to have happened in the contorted drift of Norfolk[1]. But it is only possible for this to have happened where the deposit produced a very much greater pressure upon the area which it occupies, than the rocks displaced by it did; that is when it is very much thicker than they were. And it is clearly impossible that it should elevate any displaced strata to a greater altitude

[1] "Geol. Mag.," Vol. v. p. 550.

than its own, which is necessarily limited to that of the sea-level where the deposit is going on. The test of such a mode of action would be to ascertain whether any portion of the strata, which ought to lie beneath the thick deposit, can be discovered in a contorted condition abutting upon it.

Thus far, for the sake of perfect generality, it has been assumed that the disturbed upper strata rest upon a liquid, or at least a plastic, substratum. In what follows, the enquiry will be made whether this condition of the subjacent rocks is probable. And the method taken will be to consider whether the contrary supposition, that the subjacent rocks are *solid*, will account for the amount of inequalities which the earth actually exhibits.

If the earth had cooled as a solid body, the outer layers at any epoch having attained their complete amount of contraction sooner than the interior, would have been too large to fit the interior after the cooling had proceeded further. They would therefore become corrugated. But in this case the corrugation would have necessarily taken place wholly in an upward direction, the solidity resisting the intrusion of corrugations into the lower regions; and there could be no places where any portion of the surface could have become depressed below the datum level. Hence upon this hypothesis we may introduce into our datum-level equation the supposition that $\Sigma(B) = 0$. And it becomes

$$8\pi r^2 ke = \Sigma(A).$$

Since $4\pi r^2$ is the area of the globe, it follows from this relation that $2ke$ is the average height of all the elevations above the datum level, which make up the volume $\Sigma(A)$. In other words, if all the elevations were levelled down, they would form a coating above the datum level whose thickness would be $2ke$.

The actual amount at which we may estimate this quantity is of great importance to the argument we are about to adduce regarding the physics of the earth's crust. We will now endeavour to obtain a fairly reliable measure of it from the data obtainable.

A little consideration will give the following geometrical relation:

The volume of the Sea above the datum level = the area of the whole surface of the globe × the depth of the datum level below the sea-level − the volume of rock displacing water between those levels.

Let S = area of the Sea.

D = its mean depth.

L = area of the Land.

W = volume of the Land above the sea-level.

d = the depth of the datum level below the surface of the Sea, considering these as parallel.

Now the volume of rock displacing water between the datum level and the sea-level will be $\Sigma(A) - W$, and since in the case supposed $\Sigma(A) = 8\pi r^2 ke$, we shall obtain the following relation:

$$SD = (S + L)d - (8\pi r^2 ke - W).$$

But it is to be observed that this relation assumes the surface of the ocean to be everywhere parallel to the datum level, which is itself supposed to be parallel to the original surface of the globe when it was first covered with a solid crust. But the distribution of the great continents and oceans, as well as observations on the plumb-line, render it probable that there is some cause which increases the density beneath the great oceans, and tends to accumulate the water upon them, so that their depth is greater than it would be, were that solely due to a relative depression or locally diminished curvature of the sea-bed.

Let us then call X the accumulated volume of water in excess of that which the oceans would contain, were their surface parallel everywhere to the datum level; retaining D as the observed mean depth of the sea, but using d as measured from the *supposed* surface as just defined.

In order to allow for the accumulated water, we must correct

our equation by subtracting X from the actual volume of the ocean, so that

$$SD - X = (S + L)\, d - (8\pi r^2 ke - W).$$

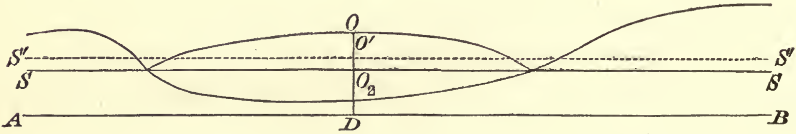
Observing that $S + L$ is the whole area of the globe, and therefore equals $4\pi r^2$, and that $\frac{X}{S+L}$ would be the depth of a layer of water of the volume of X if it were equably spread over the whole globe, which depth we will call δ, the above equation gives,

$$2ke = d + \delta - \frac{\text{vol. of sea} - \text{vol. of land}}{\text{area of the globe}}.$$

The area of the ocean is estimated to be 146 millions of square miles, and that of the land 51 millions[1].

Mr J. Carrick Moore[2] has shown that Sir John Herschel was misled by an inaccurate expression in "The Cosmos" in his estimate of the mean height of the land being 1800 feet[3], and that it ought to be put at half that, or 900 feet. The average depth of the ocean is reckoned at three miles, and its volume at 438 millions of cubic miles. By substituting these numbers we obtain

$$2ke = d + \delta - 2{\cdot}2.$$



AB the datum level.
SS the sea level supposed parallel to it.
O the actual surface of the ocean.
$S'S'$ the surface of the accumulated water levelled down.
$O_2D = d$, $O'D = d + \delta$.

1 Herschel's "Physical Geography," second edition, p. 19.
2 "Nature," Vol. v. p. 479.
3 "Physical Geography," p. 118.

Our next step must be to fix upon a value for $d+\delta$. This is the depth of the datum level below the surface of an imaginary ocean, which is rather less deep than the actual one.

Now it is not probable that there is any place where the bottom of the ocean would coincide with the datum level. Nevertheless there seems reason to think that this would approximately be the case in the deeper parts. If this be so, $d+\delta$ will not differ greatly from the measure of the deepest parts of the ocean, and it is not likely to exceed it by much, because, as already mentioned, the surface from which it is reckoned is somewhat lower than the actual surface of the ocean.

If the deepest parts of the ocean do not coincide with the datum level they must be one of two things. They must be either depressions below it, belonging to the series $\Sigma(B)$, or else they must be the places where the ocean bottom is least raised above the datum level. That they should be depressions beneath that level is scarcely possible under our present assumption that the earth has cooled as a solid; whence we have concluded that $\Sigma(B)=0$[1]. But if we take the other alternative, and suppose the ocean-bottom to be raised above the datum level, even where the depths are very great, then we are taking $d+\delta$ too small, and our estimate for $2ke$ will be too small. And this appears by far the more probable case.

On the whole, then, we may feel pretty well assured that we are within the mark if we put for $d+\delta$ the measure of the more profound parts of the ocean. The *mean* depth being taken at three miles we shall not be much in excess if we put $d+\delta$ as equal to four miles. Our value for $2ke$ then becomes

$$
\begin{aligned}
2ke &= 4 - 2\cdot2 \text{ miles},\\
&= 1\cdot8 \text{ miles},\\
&= 9504 \text{ (feet about)},
\end{aligned}
$$

a value which is more likely to be too small than too large[2].

[1] p. 52.

[2] Prof. Haughton ("Proc. Roy. Soc.," Vol. 26, p. 53, 1877) puts the area of the sea at 145 millions of square miles, and that of the land at 52 millions, and the

mean height of the land at 1000 feet. Sir Wyville Thomson, in his Rede lecture at Cambridge in 1877, stated that the mean depth of the Atlantic was about 2500 fathoms. In places its depth was 3000 fms. or a little more, and in one place 4000 fms. The greatest depth in any ocean was perhaps in the Pacific, where in one place it was 5000 fms.

If we adopt Prof. Haughton's estimate of the areas of sea and land, and of the mean height of the land; and Sir Wyville Thomson's estimate of the mean depth of the sea, viz. 2500 fms., and if we take 4000 fms. as the depth of the more profound parts (though we might apparently be entitled to take it greater), we obtain

$$2ke = 13224 \text{ feet.}$$

This value is very much larger than that which the former estimates give. But since the Author has used the lesser value in his previous publications, he thinks it better to allow it to remain, seeing that precision is not attainable, and that the larger value merely strengthens the argument drawn from the smaller.

CHAPTER VI.

ELEVATIONS ON THE HYPOTHESIS OF SOLIDITY.

Any physical hypothesis regarding the condition of the interior of the earth may be tested by comparing the average height of the elevations it would produce with that which exists in nature—Sir Wm. Thomson regards the earth as a solid cooling by conduction—Some of the results of his paper "On the Secular Cooling of the Earth"—His mathematical formula for expressing the law of temperature in descending—Calculation in general symbols of the average height of the elevations which would be produced upon the supposition of the earth cooling as a solid—Numerical result obtained with a value of the coefficient of contraction deduced from Mallet's experiments and Sir Wm. Thomson's estimates of the melting temperature and conductivity—The same with Mallet's estimate of the melting temperature of slag—The corresponding age of the earth—Amount of radial contraction.

In the preceding chapter an estimate has been made of the average height above the datum level of all the elevations upon the earth's surface, which exist at the present time, it being supposed that the most profound depths of the sea coincide nearly with the datum level. And it has been found that a very moderate computation gives 9504[1] feet for this average height: but there are good reasons for supposing that 13224[2] feet are nearer the mark. It has also been proved that upon the supposition that these elevations are the result of lateral compression, their average height above the datum level is expressed by the product $2ke$, where k is the thickness of the corrugated crust, and e the mean coefficient of compression. If then we are able to calculate the value of $2ke$ upon any physical supposition we choose to make regarding the condition

[1] p. 55. [2] p. 56.

of the interior of the earth we have a means, by comparing our result with that already approximately obtained in the case of nature, of judging whether our supposition bears the test of the comparison; it being always premised that lateral compression *is* the cause of the existence of these elevations; because it is upon this premise that our definition of a datum level, and calculation of $2ke$, have been based.

Nevertheless our estimate of the average height of the elevations above the profound depths of the sea would hold good as the expression of a fact apart from all theory.

In the second chapter it was shown that there is very little question but that the earth was at one time fluid, yet that at the present time we are compelled to admit that it is on the whole rigid: but that it is still open to question whether it is rigid throughout, or whether there is a certain level couche beneath the cooled crust where the pressure is not sufficiently great to induce solidity in rocks which, on account of the increase of temperature in descending, will otherwise be liquid. We have also pointed out how the true law of increase of temperature is inextricably mixed up with the question of the solidity or otherwise of the interior. And if the law of increase of temperature be so, it is evident that the law of cooling, upon which it depends, is also mixed up with the question of the condition of the interior; and the contraction depends on the law of cooling, and the compression on the contraction; so that all these questions are interdependent. Now the laws of cooling of a solid by conduction have been long brought under the dominion of mathematical calculation. Not so those of the cooling of a liquid by convection.

Sir Wm. Thomson having satisfied himself that the earth is as a whole *extremely* rigid, has assumed that it is *wholly* rigid from centre to surface, and has applied to it the laws of cooling of a solid by conduction. Consequently he has paved the way for an investigation of the amount of *compression*, which might accrue upon the supposition that the earth has cooled as a solid sphere. To calculate the average height of the elevations, which would have been formed up to the present time under these conditions, is the object of the present chapter.

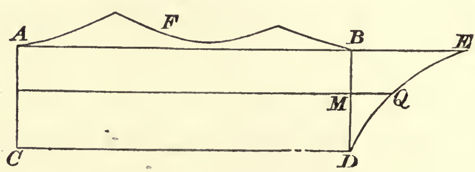
The paper by Sir Wm. Thomson "On the Secular Cooling of the Earth[1]" is of great interest and well known. From his mode of viewing the manner of its solidification, he believes that it passed from the state of a liquid to that of a solid globe in a comparatively short space of time; since which period all geological events have happened. His formula expresses the rate of increase of temperature at any given depth in terms of the length of time since complete consolidation took place. The resulting curve of temperature is of such a form that, for reasonably large values of the time in question—or of the age of the habitable world—and for all such depths as the puny efforts of man can reach, it would be impossible to perceive at the present day any deviation from a uniform rate of increase. But if it were possible to reach very great depths, the rate would be found to be sensibly diminishing; and even at the centre of the earth a greater temperature need not exist than what the uniform rate of one degree F. for 60 feet, which obtains at the surface, would bring us to at about sixty miles. The greater the value we assign to the presumed age of the world, the deeper we should have to dig before we should find the rate sensibly diminishing. Some interesting particulars concerning the relation between different assumed values for the world's age, and the depths at which it might be practicable to detect a diminution in the rate of increase of temperature, will be found in Sir Wm. Thomson's sectional address to the British Association, 1876. But the conclusion at which we must arrive is, that no mine nor bore-hole is ever likely to be put down deep enough to solve the question whether the earth is solid from centre to surface or not. If ever it could be conclusively proved by observation, due allowance being made for disturbing causes, that the rate began to diminish, that would be an argument that the earth is a solid cooling by conduction from its entire mass, and not from an internal liquid reservoir. But the shortest age that can be assigned to the world's history is too long to allow us to expect that the depth requisite to test this question can ever be reached.

[1] "Trans. Roy. Soc. Edinburgh," Vol. XXIII. Pt. I. p. 157; also "Phil. Mag.," 4th Series, Vol. XXV. p. 1, 1863, and Thomson and Tait's "Treatise on Nat. Phil.," p. 711, 1867.

Sir Wm. Thomson writes[1], "The earth, although once all melted, or melted all round its surface[2], did, in all probability, really become a solid at its melting temperature all through, or all through the outer layer, which had been melted; and not until the solidification was thus complete, or nearly so, did the surface begin to cool." The conclusion at which he arrives on these grounds, respecting the present distribution of temperature, is that the rate of increase from the surface downwards would be sensibly $\frac{1}{51}$ of a degree F. per foot for the first 100,000 feet (19 miles) or so. Below that depth the rate of increase per foot would begin to diminish sensibly. At 400,000 feet (76 miles) it would have diminished to about $\frac{1}{141}$ of a degree per foot, and so on rapidly diminishing as shown in the curve. "Such is on the whole the most probable representation of the earth's present temperature, at depths of from 100 feet, where the annual variations cease to be sensible, to 100 miles; below which the whole mass is, whether liquid or solid, probably at or very nearly at the proper melting temperature for the pressure at each depth"—that is to say, the cooling process not having at present reached so far, the state of the matter there cannot in any way influence the law of temperature in the strata above.

Having given this summary of Sir Wm. Thomson's theory concerning the condition of the interior of the earth, we will now proceed to form an estimate of the average height of the elevations above the datum level (or of $2ke$) which might be formed by the contraction of a globe so constituted.

In order to do this, we must first of all estimate the compression, kle, corresponding to the present thickness k of the crust and to a length l upon the present surface.



[1] "Nat. Phil.," p. 722.

[2] Dr Sterry Hunt supports the latter view. "American Journal of Science," Vol. v. p. 264, 3rd series, 1873.

Let $ABDC$ be a section of a portion of the crust of length l and depth k. Then e is the mean coefficient of compression for the whole, which will depend upon the depth to which the cooling down to the melting temperature under pressure has advanced. The area which by its having become compressed will have gone to form the corrugations AFB will be BDE, in which BE is the quantity by which AE has been compressed, while CD has not been compressed at all.

Then if $BM = x$, and $c =$ the compression at the depth x,

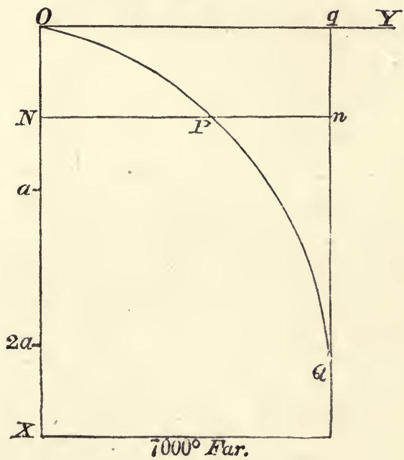
$$MQ = lc.$$

And
$$kle = \text{area } BED,$$
$$= \int_0^k c\,dx.$$

We now require to express c in terms of x.

For this purpose we avail ourselves of Sir Wm. Thomson's paper "On the Secular Cooling of the Earth," using the symbols and formulæ, and the diagram there given slightly altered, but not necessarily adhering to his values of the constants. In the event we think it will appear that we must

Diagram altered from Sir W. Thomson's paper "On the Secular Cooling of the Earth." ON the depth below the surface $= x$. NP the excess of temperature at that depth above the temperature of the surface $= v$. Oq the excess of the melting temperature above that of the surface $= V$.



adopt some modification of his views as to the condition of the globe at the time at which a rigid crust commenced to form, because we shall find that they lead us to a value of $2ke$ which is not in accordance with natural appearances.

The point which is material in the application of his investigation is, that cooling is to take place by conduction and not by convection.

V is the melting temperature of rock; which Sir William Thomson places provisionally at 7000° F., a very high estimate. Mr Mallet has shown that the temperature of slag run from an iron furnace is less than 4000° F.[1] b is a temperature such that,

$$b = \frac{V}{\frac{1}{2}\sqrt{\pi}} = \frac{7000°}{0{\cdot}88622} = 7900° \text{ F.}$$

The quantity a is thus defined: $a = 2\sqrt{\kappa t}$, where κ is the conductivity of rock, found by Sir Wm. Thomson to be "400 for unit of length a British foot and unit of time a year." And t is the number of years since the earth became solid, which must have elapsed in order that the rate of increase of temperature in descending into the earth should be what it is observed to be. Consequently t is 100,000,000 years, and $a = 80$ miles. We learn (§ *p.* of Sir Wm. Thomson's paper), that

$$NP = \frac{b}{a}\int_0^x \epsilon^{-\frac{x^2}{a^2}} dx.$$

When then the rigid crust was commencing to form, the whole globe, or at any rate its outer portion to a considerable depth, was at the melting temperature $V°$ F. represented on the diagram by Oq or Nn. At a subsequent time the temperature at the depth ON or x was $v°$, represented by the ordinate NP. Hence at that depth cooling had taken place through $(V-v)°$, represented by Pn. Now the *compression* which each layer of the crust undergoes is caused by the *contraction* of all the matter beneath it.

Let r be the present mean radius of the earth; E the cubic contraction for 1° F. Hence the layer of the sphere at

[1] "Phil. Trans. Royal Soc." 1873, Vol. 163, p. 199.

the depth x, whose area is $4\pi(r-x)^2$, will have contracted by $E4\pi(r-x)^2 dx \times Pn$; where $Pn = V - \frac{b}{a}\int_0^x \epsilon^{-\frac{x^2}{a^2}} dx$. Consequently the whole contraction from that depth downwards will be

$$E4\pi\int(r-x)^2\, Pn\, dx:$$

where the integral ought to be taken from x to the value x has at the depth at which contraction ceases to be felt. It is evident from the diagram that this is almost the case at the depth $2a$, where the temperature differs but little (by $V \times 0{\cdot}00468$[1]) from the melting temperature V.

[1] Generally

$$Pn = V - \frac{b}{a}\int_0^x \epsilon^{-\frac{x^2}{a^2}} dx$$

$$= V - \frac{b}{a}\int_0^x \left\{1 - \frac{x^2}{a^2} + \frac{1}{1.2}\left(\frac{x^2}{a^2}\right)^2 - \frac{1}{\underline{|3}}\left(\frac{x^2}{a^2}\right)^3 + \dots \right.$$
$$\left. + (-1)^n \frac{1}{\underline{|n}}\left(\frac{x^2}{a^2}\right)^n + \&c.\right\}$$

$$= V - \frac{b}{a}\left\{x - \frac{1}{3}\frac{x^3}{a^2} + \frac{1}{1.2}\frac{1}{5}\frac{x^5}{a^4} - \dots \right.$$
$$\left. + (-1)^n \frac{1}{\underline{|n}} \cdot \frac{1}{2n+1}\frac{x^{2n+1}}{a^{2n}} + \&c.\right\}$$

The general term of the series is

$$(-1)^n b \frac{1}{\underline{|n}}\frac{1}{2n+1}\left(\frac{x}{a}\right)^{2n+1}.$$

Suppose the superior limit of the integral to be $2a$. Then this becomes

$$(-1)^n b \frac{1}{\underline{|n}}\frac{2^{2n+1}}{2n+1}.$$

And the next term to this will be

$$-(-1)^n b \frac{1}{\underline{|n+1}}\frac{2^{2(n+1)+1}}{2(n+1)+1}$$
$$= -(-1)^n b \frac{1}{\underline{|n+1}}\frac{2^{2n+3}}{2n+3}.$$

And the sum of the two terms

$$= (-1)^n b \frac{2^{2n+1}}{\underline{|n+1}}\left(\frac{n+1}{2n+1} - \frac{2^2}{2n+3}\right)$$
$$= (-1)^n b \frac{2^{2n+1}}{\underline{|n+1}}\frac{2n^2-3n-1}{(2n+1)(2n+3)}.$$

There will remain the effect of the contraction of the remainder of the sphere from the depth $2a$ to the centre. This we will assume to be as great as at the depth $2a$, and then we shall be sure that the whole cubic contraction at and below the depth x is a quantity less than

$$E 4\pi \int_x^{2a} (r-x)^2 Pn\,dx + E\frac{4\pi(r-2a)^3}{3} V \times 0{\cdot}00468.$$

Suppose E' to be the mean coefficient of contraction for all the matter below the depth x. Then the whole contraction from x to the centre of the sphere will have been

$$E'\frac{4\pi(r-x)^3}{3};$$

$$\therefore\ E'\frac{4\pi(r-x)^3}{3} < E 4\pi \int_x^{2a} (r-x)^2 Pn\,dx$$
$$+ E\frac{4\pi(r-2a)^3}{3} V \times 0{\cdot}00468.$$

And the coefficient of linear compression at the depth x will be the same as the coefficient of linear contraction of the spherical surface at that depth. That is to say, $c = \frac{E'}{3}$;

$$\therefore\ c < \frac{E}{(r-x)^3}\int_x^{2a} (r-x)^2 Pn\,dx + \frac{EV}{3}\left(\frac{r-2a}{r-x}\right)^3 \times 0{\cdot}00468.$$

If we then take $2a$ as the limit of depth to which compression has reached, which we may do because the cooling below

In this expression $n=0$ will give the first and second terms of the series, $n=2$ the third and fourth, $n=4$ the fifth and sixth, &c., &c.

Making n successively 0, 2, 4,...18 (for which last value the first significant digit will not occur before the 7th place of decimals), we get for the sum of the first 20 terms

$$=0{\cdot}882083,$$

whence at the depth $2a$, $Pn = V - b \times 0{\cdot}882083$, $\left(\text{where } b = \frac{V}{\frac{1}{2}\sqrt{\pi}}\right)$,

$$= V\left(1 - \frac{0{\cdot}882083}{\frac{1}{2}\sqrt{\pi}}\right),\quad (\sqrt{\pi} = 1{\cdot}77245),$$

$$= V \times 0{\cdot}00468.$$

that has been inconsiderable, the entire compression of the crust, or ke, which is given by $\int_0^k c\,dx$, will be less than the integral of the above expression taken from the surface to the depth $2a$, that is

$$ke < \int_0^{2a} \frac{E}{(r-x)^3}\left\{\int_x^{2a} (r-x)^2\,Pn\,dx\right\}dx$$
$$+\frac{EV}{3} \times 0{\cdot}00468 \int_0^{2a} \left(\frac{r-2a}{r-x}\right)^3 dx,$$

or very approximately,

$$ke < \frac{E}{r}\int_0^{2a}\left\{\left(1+\frac{3x}{r}\right)\int_x^{2a}\left(1-\frac{2x}{r}\right)Pn\,dx\right\}dx$$
$$+\frac{EV}{3} \times 0{\cdot}00468 \int_0^{2a}\left(1-\frac{6a}{r}+\frac{3x}{r}\right)dx.$$

Let $\int_x^{2a} Pn\,dx = Q$, and $\int_x^{2a} xPn\,dx = R$.

Then

$$ke < \frac{E}{r}\int_0^{2a}\left(Q+\frac{3}{r}Qx-\frac{2}{r}R\right)dx$$
$$+\frac{EV}{3} \times 0{\cdot}00468 \int_0^{2a}\left(1-\frac{6a}{r}+\frac{3x}{r}\right)dx.$$

Performing the integrations[1], we obtain for the larger resulting terms,

$$\frac{2EV}{r}a^2 - \frac{E}{r}a^2b \times 1{\cdot}55280 + \frac{2EV}{3}a \times 0{\cdot}00468;$$

[1] It has been already proved that

$$Pn = V - \frac{b}{a}\left\{x - \frac{1}{3}\frac{x^3}{a^2} + \frac{1}{1.2}\frac{x^5}{a^4} - \right.$$
$$\left. + (-1)^n \frac{1}{\lfloor n}\,\frac{1}{2n+1}\,\frac{x^{2n+1}}{a^{2n}} + \right\};$$

$$\therefore \int Pn\,dx = Vx - \frac{b}{a}\left\{\frac{x^2}{2} - \frac{1}{3}\cdot\frac{1}{4}\frac{x^4}{a^2} + \frac{1}{1.2}\cdot\frac{1}{5}\cdot\frac{1}{6}\frac{x^6}{a^4} - \right.$$
$$\left. + (-1)^n \frac{1}{\lfloor n}\,\frac{1}{2n+1}\,\frac{1}{2n+2}\,\frac{x^{2n+2}}{a^{2n}} + \right\}.$$

and for the smaller,

$$\frac{a}{r}\left\{\frac{4EV}{3r}a^2+\frac{E}{r}a^2b\times 1{\cdot}1393-2EVa\times 0{\cdot}00468\right\}.$$

The general term of the series is

$$(-1)^n\frac{b}{a}\,\frac{1}{\underline{|n}}\,\frac{1}{(2n+1)(2n+2)}a^2\left(\frac{x}{a}\right)^{2n+2}$$
$$=(-1)^n ab\,\frac{1}{\underline{|n}}\,\frac{1}{(2n+1)(2n+2)}\left(\frac{x}{a}\right)^{2n+2}.$$

Taking $2a$ as the superior limit of the integral, the general term becomes

$$(-1)^n ab\,\frac{1}{\underline{|n}}\,\frac{2^{2n+2}}{(2n+1)(2n+2)}$$
$$=(-1)^n ab\,\frac{1}{\underline{|n}}\,\frac{2^{2n+1}}{(2n+1)(n+1)}$$
$$=(-1)^n ab\,\frac{1}{\underline{|n+1}}\,\frac{2^{2n+1}}{2n+1}.$$

And putting $n+1$ for n the next term to this will be

$$-(-1)^n ab\,\frac{1}{\underline{|n+2}}\,\frac{2^{2n+3}}{2n+3}.$$

And the sum of the two terms

$$(-1)^n ab\,\frac{2^{2n+1}}{\underline{|n+1}}\left\{\frac{1}{2n+1}-\frac{2^2}{(n+2)(2n+3)}\right\}$$
$$=(-1)^n ab\,\frac{2^{2n+1}}{\underline{|n+2}}\,\frac{2n^2-(n-2)}{(2n+1)(2n+3)}\;\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(\mathrm{A}).$$

Giving to n the series of values 0, 2, 4, 6, ..., 14, we obtain for the sum of the terms to the 16th inclusive,

$$ab\times 1{\cdot}273320.$$

Hence we have for the value of $\int Pndx$, when $x=2a$ (or at the superior limit),

$$V2a-ab\times 1{\cdot}273320;$$

$$\therefore\; Q=\int_x^{2a}Pndx=V(2a-x)-ab\times 1{\cdot}273320$$
$$+\frac{b}{a}\left\{\frac{x^2}{2}-\frac{1}{3}\cdot\frac{1}{4}\frac{x^4}{a^2}+\frac{1}{\underline{|2}}\cdot\frac{1}{5}\cdot\frac{1}{6}\frac{x^6}{a^4}-\right.$$
$$\left.+(-1)^n\frac{1}{\underline{|n}}\,\frac{1}{2n+1}\,\frac{1}{2n+2}\,\frac{x^{2n+2}}{a^{2n}}+\right\};$$

$$\therefore\;\int Qdx=\int\left(\int_x^{2a}Pndx\right)dx=V\left(2ax-\frac{x^2}{2}\right)-1{\cdot}273320abx$$
$$+\frac{b}{a}\left\{\frac{1}{2}\frac{1}{3}x^3-\frac{1}{3}\cdot\frac{1}{4}\cdot\frac{1}{5}\frac{x^5}{a^2}+\frac{1}{\underline{|2}}\frac{1}{5}\cdot\frac{1}{6}\cdot\frac{1}{7}\frac{x^7}{a^4}-\right.$$
$$\left.+(-1)^n\frac{1}{\underline{|n}}\,\frac{1}{2n+1}\,\frac{1}{2n+2}\,\frac{1}{2n+3}\,\frac{x^{2n+3}}{a^{2n}}+\right\}.$$

As yet no definite values have been assigned to any of the constants.

Taking the observed rate of cooling at $\frac{1}{51}$th of a degree F. per foot in descending, the time since the supposed solidification

The integral has to be taken from $x=0$ at the surface to that value of x which expresses the thickness of the rigid crust beyond which corrugations will not have been formed.

Now the melting temperature V is reached within an extremely small error at the depth $2a$. We may therefore take $2a$ as the superior limit, and the above quantity becomes

$$\int_0^{2a}\left(\int_x^{2a} Pndx\right)dx = V(4a^2-2a^2) - 1{\cdot}273320 \times 2a^2b$$
$$+\frac{b}{a}\left\{\frac{1}{2}\cdot\frac{1}{3}(2a)^3 - \frac{1}{3.4.5}\frac{(2a)^5}{a^2} + \frac{1}{\underline{|2}}\cdot\frac{1}{5}\cdot\frac{1}{6}\cdot\frac{1}{7}\frac{(2a)^7}{a^4} - \right.$$
$$\left. + (-1)^n\frac{1}{\underline{|n}}\frac{1}{(2n+1)(2n+2)(2n+3)}\frac{(2a)^{2n+3}}{a^{2n}} + \right\};$$

of which series the general term after simplification becomes

$$(-1)^n a^2b\frac{2^{2n+2}}{\underline{|n+1}}\frac{1}{(2n+1)(2n+3)};$$

and the next to it

$$-(-1)^n a^2b\frac{2^{2n+4}}{\underline{|n+2}}\frac{1}{(2n+3)(2n+5)};$$

and their sum, after reduction,

$$(-1)^n a^2b\frac{2^{2n+2}}{\underline{|n+2}}\frac{2n^2+n+6}{(2n+1)(2n+3)(2n+5)}.$$

This is the sum of the two corresponding terms in the former series (A) multiplied by the factor

$$a\frac{2}{2n+5}\left\{1+\frac{2(n+2)}{2n^2-(n-2)}\right\}.$$

Giving to n the values 0, 2, 4, 6,... 14, we obtain for the sum of the terms of the series to the 16[th] inclusive,

$$a^2b \times 0{\cdot}993840.$$

Hence

$$\int_0^{2a}\left(\int_x^{2a} Pndx\right)dx = V2a^2 - a^2b \times 2{\cdot}546640 + a^2b \times 0{\cdot}993840.$$

Or $$\int_0^{2a} Qdx = V2a^2 - a^2b \times 1{\cdot}552800.$$

The calculations for obtaining

$$R \text{ and } \int\left(3Qx - 2R\right)dx$$

are of a similar character.

The numerical results of these series have in every instance been verified independently from the general terms by friends.

at 7000° took place will have been 100,000,000 years, whence as already stated, $a = 2\sqrt{\kappa t} = 400{,}000$ feet, or about 80 miles. And

$$b = \frac{V}{\frac{1}{2}\sqrt{\pi}} = 7900^\circ.$$

In order to fix upon a value of E, the coefficient of contraction for 1° F., reference is made to some experiments on a large scale conducted by Mr Mallet[1]. And upon reducing his results we find for slag from an iron furnace $E = \cdot 0000215$[2], whence $\frac{E}{3}$ the linear contraction $= \cdot 000007$; which does not differ more than might be expected from the estimates made at much lower temperatures for rock by Mr Adie[3].

If we take the earth's mean radius at 20,902,520 feet, we obtain as the final result for the two first terms, 286 feet.

The third term gives the effect of the contraction of the entire volume of the sphere, from the depth $2a$ to the centre, in corrugating the crust to the depth $2a$, on the supposition that the contraction continues to the centre as great as it is at

[1] "Phil. Trans. Royal Soc.," Vol. 163, p. 201. See also "Nature," Vol. XXII. p. 266.

[2] To obtain the coefficient of contraction for 1° F. from Mr Mallet's experiments.

The cones of slag began to solidify when the containing iron cones were at 450° F. At that time the mean contents of the cones was 8231·9229 cubic inches, and the volume of the slag when measured at 53° F. was 7700·2303 cubic inches.

The slag entered the cones at a temperature of 3680°.

By Mr Mallet's estimate, founded on the *time* in which solidification commenced, the solidification commenced at about "3062°, or say 3000° F."

Hence the number of degrees through which the temperature fell was

$$3062^\circ - 53^\circ = 3009^\circ.$$

We have therefore to determine E, the coefficient of cubical contraction between incipient consolidation and 53° F.

$$7700 = 8232\,(1 - E3009);$$
$$\therefore\ E = \cdot 0000215,$$
$$\text{and } \frac{E}{3} = \cdot 0000071.$$

[3] The mean of Mr Adie's six results for the coefficient of expansion is ·0000057. "Trans. Roy. Soc. Edin.," Vol. XIII. p. 370. See also "Geol. Mag.," Vol. X. p. 260.

the depth $2a$. But no account has been taken of the corrugations which would arise from the compression of this portion itself. However Sir W. Thomson concludes that below the depth of 100 miles the earth will be at the proper melting temperature for the pressure, whilst $2a = 160$ miles. Consequently our assumption on the first account makes the effect of the cooling of this portion of the sphere enormously too great, whilst on the second account it is made somewhat too small. If we take half the calculated effect we may feel certain that we have taken it large enough. We will therefore put this term at 94 feet.

The first two terms of the bracket multiplied by $\frac{a}{r}$ give 58 feet.

And half the third gives 5 feet, and is negative.

On the whole, therefore, we may conclude that

$$ke = 286 + 94 + 53 \text{ feet},$$
$$= 433 \text{ feet}.$$

And $$2ke = 866 \text{ feet};$$

a result which is probably considerably too large.

In other words, if all the elevations which would be produced by compression, through the contraction of the earth cooling as a solid upon the above suppositions, were levelled down, they would form a coating over the whole globe of about 800 feet in thickness. This is an exceedingly small result.

We will now enquire how the result of our calculation will be affected by assuming a different value for the melting temperature, about which we have no very certain knowledge.

The function $\frac{b}{a}\int_0^x \epsilon^{-\frac{x^2}{a^2}} dx$, by which Sir William Thomson has expressed the temperature within the earth at the depth x, requires that the law of its increase shall not be affected by the sphericity of the surface; which he has shown under the circumstances to be an admissible supposition. This assumption is therefore involved in our value for Pn; and it evidently

tends to diminish the rate of increase of temperature in descending, and to make the value of $2ke$ too large, by giving too great a thickness to the cooled crust.

It is easily seen that, if we had taken our integrals from the surface to such a depth that the cooling represented by Pn becomes any given inappreciable fraction of the melting temperature, we need not have introduced the supplementary term which is multiplied by 0·00468. If we suppose pa to be the depth in question, the corresponding value of Pn would be

$$V\left(1-\frac{2}{a\sqrt{\pi}}\int_0^{pa}\epsilon^{-\frac{x^2}{a^2}}\,dx\right)$$

$$= Vf\left(\frac{pa}{a}\right),$$

$$= Vf(p).$$

This shows that the value of p, which corresponds to any assumed fraction of V, does not involve any of the constants of the problem. But Pn will not altogether vanish until the integral becomes equal to $\frac{1}{2}a\sqrt{\pi}$, which, from a known definite integral, will not be until p is infinite. We have assumed $p=2$ for convenience of calculation, since even so small a value makes Pn small. But the relation

$$ke=\int_0^{pa}\left(\frac{E}{(r-x)^3}\int_x^{pa}(r-x)^2\,Pn\,dx\right)dx$$

can be made as exact as we please by taking p large enough. Had that been done we can see that the larger terms of the result would have been proportional to EVa^2, since $b=\frac{V}{\frac{1}{2}\sqrt{\pi}}$;

and the small terms of the bracket multiplied by $\frac{a}{r}$ would have been proportional to EVa^3, a being equal to $2\sqrt{\kappa t}$. We can therefore, by means of a proportion, deduce from the value of $2ke$ already found, its value corresponding to any other values of E, V, or κ. For instance, if we introduce for V the temperature which Mr Mallet has determined for melting slag, viz. 4000° F.,

instead of 7000°, and the corresponding value for t, viz. 33,000,000 years[1], the result becomes surprisingly small indeed. For then $2ke$ would be about 149 feet.

In this case $2a$ would be about 100 miles.

It is however important to remark that as far as our determination of the average height of the inequalities is concerned the conductivity of the rock is not required to be known, nor yet the time since solidification took place, but only the value of the constant a, which is known at once from the rate of increase of temperature near the surface and the temperature of solidification. Because (see Sir William Thomson's paper)

$$\frac{dv}{dx} = \frac{V}{\sqrt{\pi\kappa t}}\epsilon^{-\frac{x^2}{4\kappa t}} = \frac{2V}{a\sqrt{\pi}}\epsilon^{-\frac{x^2}{a^2}},$$

hence near the surface, putting $x = 0$,

$$\frac{1}{51} = \frac{2V}{a\sqrt{\pi}};$$

and therefore, when $V = 7000°$,

$$a = \frac{102V}{\sqrt{\pi}} = 402832.$$

But although the actual value of the conductivity is not involved, nevertheless the equations assume that its average value continues constant, as indeed it is found to do by observation near the surface[2].

[1] To find the time since the earth was all melted, on the supposition that it has cooled as a solid, and that the temperature was then 4000° F. as determined for slag in Mr Mallet's experiments.

$$\frac{dv}{dx} = \frac{V}{\sqrt{\pi\kappa}}\frac{1}{t^{\frac{1}{2}}}\epsilon^{-\frac{x^2}{4\kappa t}}$$

$$= \frac{V}{35{\cdot}4}\frac{1}{t^{\frac{1}{2}}}\epsilon^{-\frac{x^2}{1600t}}.$$

Taking $\frac{1}{51}$ of 1° as the rate of increase of temperature at the surface where $x = 0$, we get from the above

$$t = 33 \text{ millions of years.}$$

[2] Principal Forbes found that the conductivity of iron diminishes as the temperature increases. Tait's "Recent Advances in Ph. Science," 2nd ed.,

It is desirable to know what the radial contraction of the globe would have been, that is, how much nearer to the centre any point on the surface would be at present, than it was when solidification took place. In this case we must find the contraction along a radius. But we must also remember that any point on the surface must be considered raised by the average height due to the compression which results from the sphericity of the surface, *i.e.* by $2ke$.

Now the contraction along any radius will evidently be

$$\frac{E}{3}\int_0^{2a} Pn\,dx,$$

if as before we take $2a$ as the depth to which the cooling, and therefore the contraction, is sensible. Hence the actual contraction will be

$$\frac{E}{3}\int_0^{2a} Pn\,dx - 2ke = \frac{E}{3}(V2a - ab \times 1{\cdot}273320) - 2ke.$$

If we give to V the value 7000° F., and $\frac{E}{3} = {\cdot}000007$, with the value just found for a this gives

$$\begin{aligned} \text{radial contraction} &= 11112 - 866 \text{ feet,} \\ &= 10246 \text{ feet,} \\ &= 1{\cdot}9 \text{ miles.} \end{aligned}$$

If however we assume 4000° for the temperature of solidification, since the result is proportional to Va, and therefore to V^2, we get in this case, in which $2ke = 149$ feet,

$$\begin{aligned} \text{radial contraction} &= 3479 \text{ feet,} \\ &= 0{\cdot}65 \text{ of a mile.} \end{aligned}$$

p. 264. If such be the case with the earth's crust, then the temperature would increase more rapidly, and the depth at which we might suppose the temperature of solidification to be reached would be nearer the surface. This would make the average height of the elevations less.

CHAPTER VII.

HYPOTHESIS OF SOLIDITY FAILS.

Summary of results of Chap. VI.—Grounds on which Physicists have restricted geological time within certain limits—Discrepancy between height of the actual average elevations and of those which would be produced upon a cooling solid globe—Necessary alternative suppositions—Captain Dutton's argument—Ocean basins—Opinions about them of Pratt, Mallet, Hopkins, LeConte—Radial contraction cannot explain them—Still less oscillations of surface—Examples of oscillation—N. Wales, Appallachians, New Zealand, India, Colorado Plateau—Gwyn Jeffreys on changes of level—Babbage—The distribution of elevations in ranges requires a fluid substratum—*This supposition explains other surface movements—The pressures under which any part of the crust is in equilibrium—The so-called "contractional hypothesis" not to be abandoned—Captain Dutton's objections to it considered.*

The results at which we have arrived in the preceding chapter are that, if the elevations on the earth's surface are due to the contraction of a hot solid globe, cooling by conduction to and radiation from its outer surface, then the average height of the elevations above the datum level can be expressed within the limits of error of a few feet, by a formula which we have there obtained. And if into this formula we introduce the following suppositions

(1) That the average rate of increase of temperature near the surface is 1° F. for 51 feet of descent;

(2) That the temperature at which the rocks solidified was 7,000° F.;

we then arrive at the result, that the average height of the elevations produced from the era of solidification to the present time would be between 800 and 900 feet. But if, instead of

7,000° F., we suppose that the rocks solidified at 4,000° F., which is about the temperature of melting slag, then the average height of the elevations would be less than 200 feet.

These results do not require a knowledge of the conductivity. But if the conductivity be known, then the time elapsed since solidification took place is also known, and if we accept the value for this constant found for certain rocks *in situ* at Edinburgh by Sir Wm. Thomson, viz. 400[1], it appears that it would have been about 98,000,000 years ago, if the temperature of solidification was 7,000° F.; and about 33,000,000 years if it was 4,000° F.[2] These are the grounds, and these are the data, upon the strength of which physicists have restricted geologists to a term of years for the evolution of the present state of the earth[3]. If the earth be not solid their argument fails. We have also found that with the temperature 7,000° F. the cooling would have sensibly penetrated to a depth of about 160 miles, and with a temperature of 4,000° to about 100 miles. This is irrespective of the conductivity[4]. The condition of the sphere below the depth to which cooling has extended, that is whether it be liquid or solid, would not influence the conclusions regarding the amount of the elevations arrived at in the last chapter. Its temperature there, however, must be supposed that of solidification of the surface.

[1] "Trans. Royal Soc. Edin.," Vol. XXII., 1861; 400 is nearly the mean of the 2nd column in the table at p. 426. See also "Nat. Phil.," p. 718.

[2] See p. 71, note.

[3] "If we suppose the temperature of melting rock to be about 10,000° F. (an extremely high estimate) the consolidation may have taken place 200,000,000 years ago." Sir Wm. Thomson, "Trans. Royal Soc. Edin.," Vol. XXIII. p. 157, 1862, and Thomson and Tait, "Nat. Phil.," p. 716, ed. 1867. "Geology, in framing its conclusions, is compelled to take into account the teachings of other sciences. If we felt disposed to give an indefinite amount of time to the evolution of cosmos out of chaos, as has sometimes been thought possible, the student of heat and mechanics comes in, and asserts upon the authority of his branch of science, that a limit must be put to the time available for bringing about the present condition of things: he will grant 100,000,000 to 300,000,000 of years as the extreme allowance of time; if geologists cannot be content with this allowance, a distinguished professor has said, 'so much the worse for the geologists, for more they cannot have' (Prof. Tait on 'Some recent advances in Physical Science')." Bp. of Carlisle on "Origin of the World, &c.," S. P. C. K. 1880.

[4] p. 71.

We have previously, in the fifth chapter, estimated the average height of the elevations which actually exist upon the earth's surface above the datum level, which we have assumed to coincide approximately with the profound depths of the ocean, and we have found it to be at the lowest estimate about 9,500 feet. This is more than ten times as great as the theoretical estimate belonging to the temperature 7,000° F., and about fifty times as great as the estimate corresponding to the temperature 4,000° F., upon the hypothesis of a cooling solid globe. It does not seem probable that an error in the assumed values of the temperature of solidification on the one hand, nor in estimating the average height of the actually existing inequalities on the other, can explain the discrepancy. We appear then to be compelled to accept one of the two alternatives. (1) Either the inequalities of the earth's surface are not altogether, or even chiefly, due to lateral compression, or (2) there has been some other cause involved in producing the needful amount of compression of the crust, besides the contraction of a solid interior through mere cooling.

Captain C. E. Dutton, of the United States army, has likewise noticed this inability of the contraction of a solid globe through cooling to account for the geological facts[1]. His argument is of a similar character to that of the author here republished.

The question now for consideration is whether we are right in attributing the inequalities to lateral compression, or whether there may not be some other hypothesis which better explains the phenomena.

The principal difficulties appear to be with regard to the basins of the great oceans. Were the earth a perfectly smooth spheroid, without any inequalities on its surface, even in that case an excess of density in particular regions would determine a flow of water towards them, and it is conceivable that dry

[1] "American Journal of Science and Art," Jan. 1874, Vol. VIII., 3rd series, p. 113. This article was posterior to the author's first publication on the subject, but entirely independent of it. A subsequent brochure by Capt. Dutton upon the same topic appeared in the "Penn Monthly," Philadelphia, May, 1876, which was reviewed in the "Geol. Mag." Decade II. Vol. IV. p. 322.

land and oceans might exist, even although the radial distances of the land-surfaces and of the sea-bottoms from the centre of figure might be perfectly equal. That the distribution of the oceans is to some extent actually due to such a cause appears certain; for otherwise a whole hemisphere could not be almost entirely covered with water. On this point Archdeacon Pratt remarked, "There is no doubt that the solid parts of the earth's crust beneath the Pacific Ocean, must be denser than in the corresponding parts on the opposite side, otherwise the ocean would flow away to the other parts of the earth." And after explaining the reason for this statement he adds, "There must therefore be some excess of matter in the solid parts of the earth between the Pacific Ocean and the earth's centre, which retains the water in its place. This effect may be produced in an infinite variety of ways; and therefore, without data, it is useless to speculate regarding the arrangement of matter which actually exists in the solid parts below[1]." And Herschel considered that the prevalence of land and water in two opposite hemispheres "proves the force by which the continents are sustained to be one of *tumefaction*, inasmuch as it indicates a situation of the centre of gravity of the total mass of the earth somewhat eccentric relatively to that of the general figure of the external surface—the eccentricity lying in the direction of our antipodes: and is therefore a proof of the comparative *lightness* of the materials of the terrestrial hemisphere[2]."

A like conclusion as to the greater comparative density of the bed of the ocean, was arrived at by Archdeacon Pratt from the fact that at seven Coast Stations out of thirteen, six being in the Anglo-gallic, and one in the Russian arc, it has been found that a deflection of the plumb-line exists towards the sea[3]. "In fact," he remarks, "the density of the crust beneath the mountains must be less than that below the plains, and still less than that below the ocean-bed. If solidification from a fluid

[1] "Figure of the Earth," 4th ed., p. 236, 1871. A great ice-cap would also have its effect. See Croll's "Climate and Time," Chaps. XXIII. XXIV.

[2] "Physical Geography," § 13, 1862.

[3] "Figure of the Earth," pp. 200 and 206. 4th ed. 1871.

state commenced at the surface, the amount of radial contraction in the solid parts beneath the surface of the mountain-region has been less than in the parts beneath the sea-bed. In fact it is this unequal contraction which appears to have caused the hollows in the external surface, which have become the basins into which the waters have flowed to form the ocean." Latterly, however, he attributed the formation of mountainous regions to horizontal compression[1].

Mr Mallet, in his paper on Volcanic Energy, takes a similar view. He thinks that the land- and sea-boundaries were shaped out by radial contraction during the first great stage of the operation of refrigeration, while the crust was thin and flexible, owing to the rapid contraction of its viscous portion, which must then have been much thicker than the solid sheet above it[2].

Mr Hopkins appears to have been opposed to these views which suppose a difference of radial contraction; and to have held that lateral compression was the cause of the formation of the greater inequalities as well as of the lesser ones, for we find him in discussing M. Elie de Beaumont's theories to have used these words: "The physical cause to which our author refers the phenomena of elevation—the shrinking of the earth's crust—is that which appears to me most unlikely to produce that paroxysmal action which his theory so essentially requires; and most likely to produce those slow and gradual movements which it scarcely recognizes. The actual *depressions of the great oceanic basins,* and generally the more widely extended geological depressions of the present or former periods, may, I think, be referred with great probability to this cause[3]."

The theories respecting the formation of the larger features of the earth's surface have been discussed by Professor LeConte

[1] In the third edition of the "Figure of the Earth" no mention was made of horizontal compression, but mountains were attributed to vertical expansion. In the fourth, p. 203, note, the author's estimate of the horizontal force of compression is referred to.

[2] "Trans. Royal Soc." 1873, § 52 and § 60.

[3] Presidential Address to the Geol. Soc. 1853, "Geol. Journ.," Vol. IX. p. lxxxix.

in so lucid and unprejudiced a style, that his papers are well worthy of study. The following passage conveys his conclusions. "Mountain chains and mountain ranges are therefore, I think, beyond question produced by horizontal thrust crushing together the whole rock mass, and swelling it up vertically; the horizontal thrust being the necessary result of secular contraction of the interior of the earth. The smaller inequalities, such as ridges, peaks, gorges, and, in fact, nearly all that constitutes scenery, are produced by subsequent erosion. I feel considerable confidence in the substantial truth of the foregoing statement of the formation of *mountain-chains.* As to the mode of formation of *continents* and *sea-bottoms,* I feel less confidence. It is possible that even these may be formed by a similar unequal yielding to horizontal thrust, and a similar crushing together and up-swelling. If so, it would be necessary to suppose the amount of horizontal yielding in this case much less, but the depth effected much greater than in the case of mountain-chains. But, as we find no unmistakable structural evidence of such crushing, except in the case of mountain-chains, I have preferred to attribute the formation of continents and sea-bottoms to unequal *radial* contraction[1]."

The last sentence of this passage appears to invite the remark that we cannot expect in general to have evidence of crushing except in those areas which are open to investigation, viz. on dry land. But *there* it is not confined to mountain-chains. Contorted strata are to be also found in what would be termed level countries, often covered with horizontal deposits of later date[2]: and this fact in itself proves that these contorted strata have been once covered by an ocean, offering a strong presumption that there are contorted strata now at the bottoms of the oceans.

In fact it is clear that, on account of the sedimentary character of the rocks that compose continents proving that they cannot be primæval protuberances, the only hypothesis which can compete with that of lateral compression is that of

1 "American Journal of Science," 3rd series, Vol. IV. p. 462, 1872.

2 For example the highly contorted carboniferous strata of parts of Belgium.

unequal radial contraction. But we have shown in the preceding chapter that 10,246 feet, or about 1·9 miles, would be the radial contraction of the whole thickness of the cooled crust on the supposition of the higher, and 3,479 feet, or about 0·65 of a mile, upon the supposition of the lower temperature that we have assumed.

Now we must remember that, if the oceans be due to radial contraction, it is only the *difference* of radial contraction beneath them and the land that can be appealed to, to account for their relative depression. But we see by the above that the *entire* radial contraction is not sufficient for the purpose. This seems conclusive against this cause being capable of explaining the existing difference of level between the land-surface and the ocean-bottom.

Of course it will be seen that the reasoning of the above paragraph applies only upon the hypothesis of the inequalities of the earth's surface being due to the cooling of a solid globe, or at least of one solid to the depth to which cooling has extended. There may be other causes producing radial contraction, and perhaps also expansion, of which no account has been taken.

After what has been said, it seems hardly necessary to adduce any further arguments against the production of ocean basins by radial contraction from cooling. Yet it will be well to observe that there are additional difficulties in explaining by this means the oscillations of the surface up and down, which are known to have occurred in the same areas again and again. For that land and sea might interchange places by radial contraction, the land must sink far below the bottom of the sea, so that the sea should flow into the depression thus caused, leaving its former bed forsaken and dry. This is an event which, according to our numerical results, would be quite impossible.

The more one considers the instability of the earth's crust and the magnitude according to our standards of the movements, the more one is lost in amazement: and it is impossible to avoid using the kind of exaggerated language which is the opprobrium of popular geology. Rocks of a single geological period, thicker than the heights of the highest mountains, have been deposited over certain areas, sunk, been re-

elevated, crumpled, and denuded, and still stand as lofty mountains. In North Wales, Cambrian rocks were measured by Mr Aveline of the thickness of 23,000 feet[1]. The deposits of the Appalachian chain, we are told, attain a thickness of eight miles[2]. In such cases as these, not only must the rocks now exposed at the localities, where the lower beds of the series are at the surface, have been at one time as far below the sea; but the areas now occupied by sea must have presented land surfaces, in the latter instance believed to have been to the north-east, in order to furnish the detritus out of which these great piles of strata were formed. Again the islands of New Zealand occupy a central position in the aqueous hemisphere; and yet they contain a series of deposits chronologically analogous to those of the northern hemisphere[3]; whence it follows that they must have been often submerged during periods, when there existed, in what is now an extended ocean, land surfaces, from which the materials of their rocks must have been derived.

The Himalayan area presents some peculiarly interesting features in this connection. The sub-Himalayan range consists of tertiary strata which are now highly disturbed. All, with the exception of the lower portion, which is nummulitic and consequently marine, are composed of sub-aerial deposits, formed by detritus brought down by torrents from the Himalayas. These deposits are together between 12,000 and 15,000 feet thick. The sandstones, which form the chief portion of these beds, and the red clays which are intercalated with them, are exactly like the alluvial deposits of the plains. "Thus it was suggestive, and not altogether misleading, to say that the Siwaliks were formed of an upraised portion of the plains of India[4]." The surface movements indicated by this history suggest a level area at the foot of the Himalayan range, sinking continuously during the former part of the tertiary period. Then a great movement of

[1] Jukes' "Student's Manual of Geology," 3rd ed., p. 526, 1872.

[2] Hall's "Natural History of New York," Part VI. Palæontology, Vol. III. p. 67.

[3] Capt. F. W. Hutton, "Geol. Mag.," Decade II. Vol. I. p. 27, note. See also Hochstetter's "Geology of New Zealand," Auckland, 1864.

[4] "Manual of the Geology of India," Medlicott and Blandford, p. 525.

lateral compression and elevation took place. Again it sunk, and unconformable beds were deposited. These were again elevated and compressed. Such at least appears to be the interpretation of the description given by the surveyors[1]. But the point which is material to our present subject is the sinking of the land surface to the depth of nearly three miles, while river deposits to that thickness were being laid down ; the whole being denuded off mountains whose spoils have in more recent times provided materials for the great plains of India ; and still those mountains stand the highest in the world. That a sinking of the area of the plains of a similar character is yet in progress, is shown by the boring at Fort William, near Calcutta, in which to the depth of 400 feet fresh-water deposits occurred. The conclusion seems irresistible that corresponding to the long, though occasionally interrupted, depression of these plains, a correlative elevation of the great range which has supplied the deposits has been going on.

This sinking of river plains and estuaries is an apparently universal occurrence: the great thickness of fluviatile deposits in them cannot otherwise be accounted for. Thus the fluviatile accumulations have been proved in the case of the Po to the depth of 400 feet, of the Ganges to 481, and of the Mississippi to 630 feet[2].

The recent explorations of the region of the Rocky Mountains by the Government Survey of the United States, has greatly added to our knowledge about the movements of the earth's crust both in kind and in degree. The rocks affected are of many periods, down to the newer tertiary, and even the quaternary[3]. They are cut up by enormous faults into blocks, the throw of the faults being sometimes as much as 40,000 feet. Yet the movements have been so gradual that the rivers have had time to deepen their channels as the plateaux rose, so that they now flow in the profound "Cañons of the Colorado" sometimes more than a mile deep.

[1] See Diagram, p. 44.

[2] Dr Ricketts, "Valleys, Deltas, Bays, and Estuaries," p. 22. Liverpool, 1872.

[3] "High Plateaus of Utah." Capt. Dutton, Prefatory note by Major Powell, p. viii. See a review of this work by Prof. A. Geikie in "Nature," Vol. XXII. p. 324.

Reference has already been made to the opinion that the continents have occupied nearly their present positions from the earliest times. It is doubtful whether there are sufficient grounds to render this doctrine quite certain. In spite of the advance which has been made within the last few years in our knowledge of the nature of the ocean floor by dredging and sounding, we must probably always lack experimental knowledge as to whether it covers submerged land or not. Dr Gwyn Jeffreys has described some facts regarding the occurrence of marine shells of existing species at different heights above the present level of the sea, showing how recently much of the present land has been raised. And he has asked, "Can we rightly assign to the present oceans that geologically remote antiquity which is claimed for them[1]?"

Enough has been said to show that difference of radial contraction from mere cooling is utterly inadequate to account for the existing inequalities of the surface, and still more so for the oscillations which have affected those surfaces during all geological periods. The theory of Babbage[2] that the heat, conducted upwards into the thick deposits laid down at the bottom of the ocean, expands and elevates their surface, is possibly a *vera causa*, but quite incapable by itself of explaining great changes of level. Moreover the heat conducted into the new deposits must be abstracted from the couches beneath, so that there can be no absolute increase in the amount of heat beneath the area in question except such as is supplied to it laterally, so that the process must be excessively slow.

We have previously been reasoning upon the hypothesis that a solid globe, in the process of cooling, might, owing to the compression of the crust, protrude a certain quantity of the matter of the crust above what would have been its surface had the crust been perfectly compressible—*i.e.* above our "datum level." But are we justified in supposing that that matter

[1] "Quart. Journ. Geol. Soc." Vol. XXXVI. p. 351. See also a paper by Mr T. Mellard Reade, "Geol. Mag." Decade II. Vol. VII. p. 385.

[2] On the Temple of Serapis, "Geol. Journal," Vol. III. p. 186; a paper containing the germ of much true reasoning about the physics of the earth's crust.

would have been so distributed as to have formed continents and their back-bone mountain ranges? Unless the material of which the crust was composed was extremely unhomogeneous, and unhomogeneous in strips corresponding to the axes of elevation, and unless these strips changed their positions and directions at different periods, it is impossible that such compression can have produced the present distribution of the elevated ranges; much less different distributions at different epochs. *What is required to explain the phenomena is a liquid, or at least a plastic substratum for the crust to rest on, which will allow it to undergo a certain amount of lateral shift* towards the ranges.

Neither is it easy to explain the sinking of areas in proportion to their becoming overloaded with sediment, whether beneath the ocean or sub-aerially, upon any other supposition. But if such a liquid substratum be granted, many of the facts are more easily explained. In that case the semi-rigid crust would assume a position of rest upon this substratum, which, within certain limits depending upon its rigidity, would be one of hydrostatic equilibrium. That is to say, the distribution of load might be made *somewhat* different from what it is without disturbing the equilibrium, but it could not be made *very* different. For instance, if the present configuration of the Himalayan region be one of approximate equilibrium, which it clearly is, if much sediment is brought off the mountains and spread over the plains, the mountains become after a while too light and the plains too heavy, and accordingly the mountains rise and the plains sink to restore the contour. This appears to be what has happened.

The pressures under which any portion of the crust would be in equilibrium will be (1) the tangential stress, (2) its own weight, and (3) the upward pressure of the substratum. If any one of these is changed beyond a certain amount the equilibrium will be destroyed and a movement of the crust will ensue.

Here are three forces involved, each of which is due to a different proximate cause: the tangential pressure producing compression of the crust (from whatever arising); the distribution

of the weight of the crust, which will depend upon the transfer of sediment, and is therefore caused partly by solar energy; and pressure from beneath, which may be attributed to elastic matter confined by the superincumbent crust.

No true theory of crust movements can be propounded, which does not involve all these simultaneously. But it is hardly unfair to say that in general this consideration has been overlooked, and one or other, or perhaps two together of these causes, have been invoked, and the theory has been incomplete accordingly.

We henceforward assume the existence of a substratum which is either continuously and continually fluid, or at least plastic. It might be supposed that a stratum whose solidity is due to pressure, and which becomes plastic when the pressure is lessened, would satisfy the requirements of the problem. But if it be true that the overloading of an area with sediment causes it to sink, this cannot be the case; because by that means the pressure, and therefore the solidity, ought to be increased instead of diminished; and the crust would not yield to the pressure.

The possibility of the existence of such a liquid substratum as is postulated, has been explained in the second chapter. We now perceive that its existence is *necessary* for the explanation of the phenomena of geology. We no longer regard the globe as solid from surface to centre. But we do not on that account give up what has been called the contractional hypothesis, but rather seek other causes adequate to produce the compressing stress, which we believe has been one of the forces to which the production of the inequalities of the surface is due.

It will be as well at this point to refer to some objections raised by Captain Dutton against this hypothesis, and if possible to reply to them. "The displacements which the strata have suffered are frequently extreme. Not only are they buckled up into great wave-like ridges; but are frequently inclined past the vertical, and are sometimes turned almost completely upside down. In New England and the Middle States the palæozoic strata are so extremely flexed and the folds so closely pressed together that they present in many

localities nothing but a series of beds all dipping to the south-east at a high angle. Yet in spite of the extreme displacement there is no chaos. The different beds are not crushed into fragments nor disorganised, but preserve their relative positions as perfectly as when they were deposited. However vast the disturbing force must have been, we may well wonder at the gentleness and ease with which they have been lifted up or let down. As if to remind us how destructive the force might have been, we find here and there a few acres which have unmistakably been subject to lateral thrusts in consequence of the sliding of a large mass down a steep incline, or some other local cause, and the strata have 'gone into pi.' This preservation of continuity would suggest to the investigator who might endeavor to apply mechanical principles to the problem, that the force which produced the movements was a *minimum* force—that is, a force having the smallest intensity which is capable of producing the movement. But this is demonstrably a system of forces acting upwards at the anticlinals and downwards at the synclinals. It is equally capable of demonstration that of all possible modes in which a force could be applied to produce a fold, the horizontal or tangential application would require the greatest intensity. It is the latter force which the contractional hypothesis supplies. It is difficult to admit that it could produce plications at all: the most probable result of it would be the annihilation of all traces of structure and stratification. This inference will be strengthened by recalling a well-known law of mechanics that tendencies to rupture ('moments of rupture') increase with the cubes of dimensions, while resistances to rupture ('moments of resistance') increase only with the squares. The masses under consideration are, collectively, of the extent of states and empires; the individuals are mountain ranges and valley bottoms, and their coherence in the presence of the forces which are adequate to move them becomes by virtue of the foregoing law a vanishing quantity. Such masses, under the action of the supposed force, would be the merest rubble, and quite incapable of preserving their integrity. The action has been frequently illustrated by subjecting a pile of paper to compression edgewise.

A closer analogy would be presented if the paper were reduced to ashes or charcoal before applying the pressure. A better, though far from an adequate one, may be found in the chaos produced in the Arctic regions when a great ice-floe is driven upon a rocky coast[1]."

The former part of this quotation gives a very graphic description of the phenomena which we have to explain. But the objection is not perhaps of great force. In the first place, it is difficult to understand how the extreme flexure, with folds so closely pressed together as to present a mere series of beds dipping all in one direction, can be produced by any conceivable "system of forces acting upwards at the anticlinals and downwards at the synclinals." Nor yet does it seem necessary that a horizontal compressing force should reduce the strata to the "merest rubble." Such a force would accumulate until the beds yielded along some line. But when that had happened the movement would not become any the more rapid. The compression would be capable of attaining great intensity, but need not be impulsive, and would quickly cease when satisfied.

Some very interesting experiments upon the effect of compression upon layers of clay have been made by Professor A. Favre, of Geneva, the results of which closely imitate the contortions seen in real mountains. Some of them are figured in "Nature[2]." The original *quasi* stratification of the clay experimented on is nowhere obliterated. It has not "gone into 'pi.'"

[1] Theories of the Earth's Physical Evolution, "Penn Monthly," 1876, p. 376. See also "Geol. Mag." Dec. II. Vol. III. p. 327.

[2] "Nature," Vol. XIX. p. 105. See also chap. X.

CHAPTER VIII.

FLUID SUBSTRATUM.

The extravasation of water substance from beneath the crust suggested as a cause of contraction of volume—Igneo-aqueous fusion—Dissolving power of water when above the critical temperature—Scrope—LeConte—Escape of steam from volcanic vents—Opinion that sea water gains access to volcanic foci—Objections to this hypothesis—Experiment of Daubrée not in point—Views on cosmogony—Dr Sterry Hunt's theory—Objections to it—Fails to account for the facts—A suggestion to explain the presence of water substance below the cooled crust.

We have now to seek for causes of compression by contraction of the volume of the globe beneath the cooled crust, or otherwise; and we have arrived at the conclusion that we are at liberty, or rather are obliged, to seek them compatibly with the existence of a liquid or plastic substratum. Accordingly, when the Author's paper on "the Inequalities of the Earth's Surface viewed in connection with the secular Cooling" was read, it contained the following suggestion:

"There may have been a considerably larger nucleus inclosed within the crust in early times than we have at present, and a great portion of such nucleus may have consisted in superheated water, the rocks being in a state of igneo-aqueous solution: and much of this water may have been blown off in steam during volcanic eruptions, by that means materially contributing to the diminution of the volume. I have suggested in my former paper read before this Society, that Mr Sorby's observations on the water enclosed in granitic crystals, along

with crystals of chlorides, renders it probable that the steam emitted in eruptions may be a constituent part of the deep-seated rocks, for it is probable that but a small part of the water contained in any magma would become confined in the interior of the crystals[1].

"Here, however, the question arises whether it would be possible for a crust to form over a layer of molten rock in a condition of igneo-aqueous fusion. Would not the escape of the water cause a state of constant ebullition which would prevent the formation of any crust until it had ceased through the escape of all the water?

* * * * * * * * *

"We can conceive that at depths where the heat and pressure were sufficient there might be no tendency to evaporation and consequent ebullition, so that after the water had escaped to a certain depth ebullition would cease, and a crust be formed; but that more water would be ready to separate to a greater depth when its affinity for the rock became lessened through the abstraction of heat, or diminution of pressure owing to the crust being partially supported by corrugation.

"If such was the condition of the interior in the early stages of the cosmogony, a large portion of the oceans now above the crust may once have been beneath it, and thus we gain a novel conception of a sense in which the fountains of the abyss may once have been broken up[2]."

Upon this passage the Referee made the following annotation.

"The Author suggests other causes of shrinking besides loss of heat, namely the escape of water.

"It is probable, or rather certain, that water substance, if it exists at great depths under great pressure and at high temperature, is neither a gas nor a liquid, being above its critical point.

"In this state substances are easily dissolved in it, not however so much on account of a greater tendency to combine with water, as on account of a greater tendency of their own to

[1] "Trans. Camb. Phil. Soc." Vol. XII. Pt. 1, p. 505.

[2] "Trans. Camb. Phil. Soc." Vol. XII. Pt. 2, p. 431.

dissipation. At still higher temperature the water substance becomes itself dissociated into oxygen and hydrogen. But it does not follow that the dissolved substances will be precipitated. The magma may be all the more complete the higher the temperature, because, though the bonds of affinity have fallen away, the prison-walls prevent the elements from escaping. But of all the known regions of the Universe the most unsafe to reason about is that which is under our feet."

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"There is a suggestion as to shrinkage by escape of water, the objections to which so far as they are stated in the paper"—*viz.* those in the second paragraph on the last page—"I do not think of great moment, considering the slowness of diffusion through a thickness equal to that of the earth's crust[1]."

This suggestion of the escape of water was subsequently repeated in a paper upon "Mr Mallet's Theory of volcanic energy," in the *Phil. Mag.* for October, 1875: and upon receiving a copy of it, Mr Scrope wrote: "There is one of the points you put forward which never struck me before, but which now appears to me most valuable; namely, that the enormous amount of steam that has escaped from the interior in early times, as well as down to the present, has been, and is, the cause of those *subsidences* of the crust, to which the basins of seas and oceans, and the crumplings of the terrestrial rocks are owing, far more than to any general contraction of the nucleus

[1] "The general result obtained from these experiments was, that the solvent power of water was found to be determined by two conditions: 1. Temperature, or molecular *vis viva;* and 2. Closeness of the molecules on pressure, which seems to give penetrative power. From these observations it will be seen that, if a body has any solvent action on another and does not act upon it chemically, such solvent action may be indefinitely increased by indefinitely increasing the temperature and pressure of the solvent. In nature the temperature has been at one time higher than we can obtain artificially, and the pressure obtained by a depth of 200 miles from the surface is greater than can be supported by any of the materials from which we can form vessels. It will thus be seen that, whereas in nature almost unlimited solvent power could be obtained, we are not as yet able to reproduce these conditions artificially. Could pressure alone increase solvent power, then much might be done, but pressure only acts by keeping the molecules close together when they have great *vis viva;* and this latter is only obtained by high temperature."—Hannay on the Artificial Formation of the Diamond. Paper read at the Royal Society. See "Nature," July 15, 1880.

by cooling." The hypothesis has also been favourably mentioned by Prof. Joseph LeConte[1]. Under these circumstances it appears worth while to follow it out somewhat further, and to examine its validity.

There can be no question regarding the fact, that enormous quantities of steam are emitted from volcanos when in eruption; and some amount almost continually from many. To be assured of this, it is sufficient to look at the frontispiece of Scrope's "Volcanos," or at the view, copied from a photograph, of Vesuvius on the 26th April, 1872, in Palmieri's "Vesuvius," translated by Mr Mallet[2]. That intrepid observer relates, on that day "two large craters opened at the summit, discharging with a dreadful noise, audible at a great distance, an immense cloud of smoke and ashes, with bombs and flakes, rising to the height of 1300 metres above the brim of lava." The Author remembers that in the narrative of a correspondent of a newspaper at the time, the sound, as heard at Naples, was compared to four lions roaring in the four corners of the room in which he sat. These and similar descriptions give a notion of the enormous quantities of steam, and of the force with which it is emitted on these occasions. The question is, not regarding the fact that water is present in the volcanic emanations, but how it comes to be there.

This of course opens up the whole subject. The circumstance that many of the best-known volcanos are near the sea, that many actually form islands in the wide ocean, and that great trains of volcanos skirt the shores notably of the Pacific, has led to the opinion that the waters of the sea by some means find access to what have been called the volcanic foci, and are there heated, and find exit through their vents. This explanation has been thought to receive support from the "sea salts," which are deposited by the emanations[3].

There are but two ways in which the waters of the sea

[1] American Journal of Science and Arts, Vol. XVI. p. 107, 3rd Ser. 1878.

[2] Asher and Co., London, 1873. See Plate IV. A., and p. 91.

[3] "Even chloride of iron, which was so abundant in the lavas, was scarcely perceptible in the smoke, which almost exclusively deposited sea-salt on the surrounding rocks; I say sea-salt advisedly, and not chloride of sodium, to show that I include all that sea-salt contains." Palmieri, Op. cit. p. 121.

could possibly find access to the hot interior; and these are, by open fissures, and by capillary absorption. If a fissure were to open at the bottom of the sea, so that water were to gain access to heated rocks, it seems incredible that the steam, if such were formed, should not be formed at once, and the water be forced back again through the same fissure by which it entered. Or if we suppose, as is probable, that the pressure of the water would prevent the formation of steam, still there would be a highly expansive fluid, which would rush upwards at the same place, rather than traverse a long underground journey to find exit at a neighbouring volcanic vent.

The more plausible opinion is, that the water gains access by capillary absorption, and slowly accumulates in the supposed "volcanic foci," until at last it bursts forth in an eruption. Capillary attraction can be made to do great things, as for instance to split blocks of granite, by driving in dry plugs of wood and wetting them. But it cannot cause a liquid to flow continuously through a tube, however short; for, if it could, it would give us perpetual motion. And after all it is a finite force and requires special conditions for its development. The chief of these is that surfaces of three media should meet two and two in contact. Thus when water rises in a capillary tube of glass, we have the three media of glass and water and air. The tension of the surface separating water and air is greater than that of the surface separating water and glass[1]. In the present case, there is no third medium present, vapour of water, which might act as such, being prevented from forming by the pressure, so that it does not appear how the action can be set up. If there were a cavity filled with vapour, it is possible that the density of that vapour, and therefore its pressure, might be increased to a certain extent, by the evaporation of the water from the extremity of the capillary tubes, and that was what occurred in the experiment of M. Daubrée[2]. But under the conditions

[1] See Maxwell's "Heat," 2nd ed. p. 286.

[2] M. Daubrée (*vide* "Rapport sur les progrès de la Géologie expérimentale," Paris, 1867) deduced a theory of volcanos from the following experiment. He separated two chambers by a circular plate of rock two centimetres in thickness. The upper chamber communicated freely with the atmosphere, the plate of rock forming its lower face. The second chamber was empty, and communicated

present the pressure is too great for the formation of vapour, and the temperature too high for capillary attraction to be developed[1].

Still further the existence of capillary communication of water from the surface may be doubted. For if there were supposed a capillary tube extending from the bottom of the ocean, the pressure at the lower end of this tube would be that of the water contained in it *plus* that, if any, arising from capillarity, while the pressure of the crust around its mouth would be that due to the weight of the crust. This latter would be the greater of the two, consequently the liquid upon which the crust rested, having a tension equal to the weight of the crust, would force back the water in the tube, and if it were not too viscous would itself occupy the tube. Thus it appears that the waters of the ocean cannot supply the steam emitted from volcanic vents. The conclusion seems inevitable that water substance holding sea salts in solution, is an original constituent of the magma from which these vaporous emanations are derived. What is this magma?

with a manometer. The entire apparatus having been raised to 160°, water was poured into the upper chamber; and soon afterwards the manometer was found to indicate two atmospheres of pressure. When a portion of the vapour accumulated in the lower chamber had been allowed to escape, the pressure soon recovered its former value. There was then a true feeding across the partition of rock, the cause of which was the desiccation of the lower face of the partition by vaporization of the water in its interstices, and the subsequent replacement of that water by capillary attraction. (Capillary attraction acted the part of the force-pump which feeds a high-pressure boiler.)

M. Daubrée conceives that if the layer of rock were of great thickness, and a very high temperature maintained in the cavity, a correspondingly high steam-pressure would result, which would be sufficient to raise lava in the vent of a volcano, and to produce earthquakes; while the force so obtained might after expenditure be again and again renewed.

This theory requires the occurrence of cavities at great depths ("supposons une cavité séparée des eaux de la surface") communicating with the volcanic vents. But the only argument in favour of cavities existing seems to be that the requisite mechanical force is supposed obtainable by means of them; but it seems *à priori* impossible that there should be such cavities.

[1] "In all liquids on which experiments have been made the superficial tension diminishes as the temperature rises, being greatest at the freezing point of the substance, and vanishing altogether at the critical point where the liquid and gaseous states become continuous." Maxwell's "Heat," 2nd ed., p. 290.

There is a natural unwillingness among Geologists to involve themselves in speculations concerning the cosmogony. This is due partly to the inherent uncertainty of the subject, and partly to the deeply implanted doctrines of the uniformitarian school, which in effect teaches that there are no grounds for sound reasoning upon any state of things different from what we now see. However, we are obliged to form some theory on this question if we would speculate on the constitution of the depths beneath the cooled surface of the globe.

Among those who have written upon the question, Dr Sterry Hunt, of Montreal, has perhaps offered the most plausible explanation. Impressed with the necessity of accounting for the presence of water in the volcanic laboratory, and assuming that all the water belonging to this planet must have existed originally as a gaseous envelope surrounding a still incandescent solid ball, he appears to have seen no means of accounting for this subterraneous water, except by supposing it to have been subsequently derived from the atmosphere by precipitation.

"While admitting with Hopkins and Scrope the existence of a solid nucleus and a solid crust, with an interposed stratum of semi-liquid matter, I consider this last to be, not a portion of the yet unsolidified igneous matter, but a layer of material which was once solid, but is now rendered liquid by the intervention of water under the influence of heat and pressure. When, in process of refrigeration the globe had reached the point imagined by Hopkins, when a solid crust was formed over the shallow molten layer which covered the solid nucleus, the farther cooling and contraction of this crust would result in irregular movements, breaking it up, and causing the extravasation of the yet liquid portions confined beneath. When at length the reduction of temperature permitted the precipitation of water from the dense primæval atmosphere, the whole cooling and disintegrating mass of broken up crust and poured out igneous rock would become exposed to the action of air and water. In this way the solid nucleus of igneous rock became surrounded with a deep layer of disintegrated and water-impregnated material, the ruins of its former envelope, and the chaotic mass from which, under the influence of heat from

below and air and water from above, the world of geologic and of human history was to be evolved[1]."

And further on he explains the relation, which this supposed reconstituted outer layer holds to his volcanic theory.

"Considering the conditions of its formation, water would seem to be necessarily absent from the originally fused globe, in which the older school of Geologists conceive volcanic rocks to have their source. Scheerer supplemented Scrope's view by showing that the presence of a few hundredths of water, maintained under pressure at a temperature approaching ignition, would probably suffice to produce a quasi-solution, or an igneo-aqueous fusion of most crystalline rocks; and subsequent observations of Sorby have demonstrated that the softening and crystallization of many granites and trachytes must have taken place in the presence of water, and at temperatures not above a low red heat. Keeping in view these facts, we can readily understand how the sheet of water-impregnated débris, which as we have endeavoured to show must have formed the envelope to the solid nucleus, assumed in its lower portion a semi-fluid condition, and constituted a plastic bed on which the stratified sediments repose. These sediments which are in part modified portions of the disintegrated primitive crust, and in part of chemical origin, by their irregular distribution over different portions of the earth determine after a lapse of time, in the regions of their greatest accumulations, volcanic and plutonic phenomena. It now remains to show the observed relations of these phenomena both in earlier and later times to great accumulations of sediment[2]."

On these passages the following remarks suggest themselves. The first process towards obtaining "a deep layer of disintegrated and water-impregnated material, the ruins of the former envelope" of the solid nucleus, is supposed to arise from "the further cooling and contracting of a solid crust formed over a shallow molten layer."

Let us consider this point. The solid crust supposed may

[1] Sterry Hunt's "Lecture on Volcanoes and Earthquakes," p. 6. This short lecture, without date or name of publisher, contains a clear resumé of the writer's views. [2] *Ibid.* p. 8.

be compared to the surface of a lava stream. When once this surface had attained the temperature of the atmosphere, it would contract no further, and the hotter subjacent parts would cool by conduction through the exterior. It does not appear therefore that gaping cracks would be formed in it, but on the other hand corrugations, the lateral pressure rather hindering extravasation than promoting it. But this does not appear to be the condition that Dr Hunt contemplates for the formation of a water-impregnated layer.

The proximate cause of mountain elevation he considers to be the depression of this supposed water-impregnated layer through the covering of it by thick deposits of detritus, in consequence of which the isogeotherms have risen, the water-impregnated layer has entered on a state of igneo-aqueous fusion, and the globe *contracting by general loss of heat* has caused the crust to be wrinkled along the elongated areas in this manner weakened.

It is obvious however, that the theory thus developed, admirable in some respects, offers no special facility towards affording sufficient compression. The contraction of the matter of the globe through mere cooling is alone appealed to. All the objections therefore to the adequacy of this cause, which have been brought forward in the previous chapters, apply with equal force to Dr Hunt's explanation. It may also be questioned whether a layer of material simply deposited from water, in a manner similar to that of the ooze or sand of the ocean bed, would after consolidation into rock supply sufficient water to account for the immense quantities of steam given off during volcanic eruptions. It appears that there ought to be, under this view of its derivation, the same equivalence between the amount of the lava and of the gaseous emanations from a vent, as between the earthy sediment and the water incorporated with it when it became consolidated after precipitation. Although no exact measures are known, still it seems that the water given off in steam bears too large a proportion to the solid matter to be thus accounted for; especially when we remember that, besides the emanations from the crater, a large quantity of steam is emitted by the lava stream after eruption, before it becomes cool.

The presence of so large a quantity of water substance in the deep-seated and molten rocks is still to seek.

The following supposition is offered with considerable diffidence. It has been suggested by a paper lately read by Mr Mallet before the Geological Society[1], although it will be apparent that it is not the same theory as Mr Mallet's. That gentleman has pointed out the undoubted fact, that, if all the water upon the face of the globe were to be at the present moment converted into vapour, the pressure which it would exert at the earth's surface would be the same as would be exerted by the present oceans, were they spread in an equable layer over the surface. He then enters into speculations regarding the temperature to which water might be raised under such pressure, and its effect upon rocky matter.

Let us then revert to the far distant time when the temperature of the earth had fallen to that point, when the oxygen and hydrogen were first able to combine. The pressure, caused by the gravitation of the water substance formed from them, would have been that due to a layer of liquid water its exact equivalent. This would have been the same as the pressure of a column of an ocean somewhere about two and a half miles deep. The water substance would at that time have been at far above its critical temperature, and possessed of great solvent power upon silicates and some other minerals. It may then be asked, Is it necessary to suppose that the rocky materials had already formed a solid globe, upon which this water substance was supernatant? Might not rather such substances as were soluble have been in solution with it, even to that depth at which the magma was succeeded by denser materials insoluble in superheated water? If that were so, then as cooling proceeded a crust of rock would have been formed by crystallization at a certain level, and would have gradually extended downwards. But there may be even still remaining an intensely hot layer of original water-dissolved silicates, underlying the solid crust, ever in readiness to furnish the steam, gases, and ejectamenta of the volcano; and by the extravasation especially of the former of these to contribute to the contraction of volume of the globe.

[1] "Quart. Journ. Geol. Soc." Vol. XXXVI. p. 112.

In our ignorance of the properties of water at the high temperatures and under the pressures we are now considering, it is not possible to arrive at any but the most general conclusions.

If we take the mean depth of the ocean to be 2500 fathoms, we find that the pressure due to a layer of water of this depth would be 442 atmospheres, or 336424 mm. of mercury. But we must reduce this in the proportion of 146 : 197[1], because the depth will be inversely proportional to the area, and we are now supposing the ocean spread over the whole globe. This will make the pressure about 327 atmospheres or 249329 mm. of mercury. We do not know what temperature corresponds to this pressure. It was found by M. Cagniard de la Tour that the critical temperature of water is about 773° F., but the pressure was not determined. At that temperature the water began to dissolve the glass tube which contained it. At the temperature 230° C., or 446° F. the pressure of steam is 20926 mm. of mercury[2]. This appears to have been the highest determined by Regnault.

It is at once apparent that the temperature corresponding to 249329 mm. of mercury must be greatly above the critical temperature.

The lower portion only of the aqueous envelope would have been sufficiently compressed to hold silicates in solution. These would have begun to be deposited in a solid and probably

[1] Ch. v. p. 54.

[2] Balfour Stewart's "Heat," 2nd ed. p. 402. Locomotive engines are constructed to work up to a pressure of about 10 atmospheres.

Extract from a letter by Mr J. Aitken in "Nature," vol. XXIII. p. 34, 1880 :—"A *free surface* is any surface of the body under examination at which it is *free* to change its state.". . . . "As to what the freezing, melting, and boiling points are when these *free surfaces* are absent, we have at present no knowledge whatever. All we know is, that the freezing point is lower, and the "melting" and "boiling points" are higher, than when *free surfaces* are present. The first of these points is too well known to be referred to here. The last point was illustrated in the paper referred to" (Royal Scottish Society of Arts, 1874–5) "by an experiment in which water was heated in a metal vessel under atmospheric pressure to a temperature far above 'the boiling point,' when the water exploded and violently ejected itself from the vessel. The superheating of the water was accomplished by carefully excluding all *free surfaces*, by bringing the water into as perfect contact with the metal of the vessel as possible."

crystalline state before any rain could fall from the atmosphere, because precipitation of water could not take place until the temperature had decreased to about 773° F., and before that the water substance must have lost the greater part of its solvent power on the earthy salts. The sea salts would have remained in solution in the primitive ocean after the silicates had crystallized out, and they would still continue to form a constituent part also of the hot magma supposed to exist beneath the solid crust in a state of igneo-aqueous solution.

This magma is probably of the nature of an elastic liquid, for, "above the critical temperature for the substance no amount of pressure will produce the phenomenon which we call condensation[1]."

[1] Maxwell's "Heat," 2nd ed. p. 119.

CHAPTER IX.

CRUST NOT FLEXIBLE.

The "datum level" equation has served an important purpose—Supposition of a flexible crust—Mode of determining the general character of the undulations which a thin flexible crust would assume when resting upon a liquid—Mathematical investigation of the problem—Certain negative conclusions therefrom regarding the earth's crust—Alternative assumption to which we are led.

We have thus arrived at the conception of an elastic highly heated layer of rocky matter[1] combined with water substance, kept in a state of compression by the superincumbent pressure of the crust, but ready to burst forth with the evolution of steam and gas wherever and whenever a vent is opened for its escape.

We are now freed from the trammels imposed by the hypothesis of a solid globe, contracting from the effect of cooling only. In reverting to our original relation between the horizontal compression and the volumes of the inequalities—which we have called our datum level equation—viz.

$$8\pi r^2 ke = \Sigma\,(A) - \Sigma\,(B),$$

we are no longer obliged to consider $\Sigma\,(B)$ as non-existent: for it is now conceivable that the solid crust may dip downwards into the fluid substratum, which in its turn may rise into the anticlinals.

It is true that under our new hypothesis this equation is indeterminate, because we do not know where to place the

[1] This layer will be usually termed "fluid" rather than "liquid," because the term "liquid" does not convey the idea of elasticity which, when the pressure is sufficiently diminished, will be an attribute of the substratum.

datum level. But it has already served a most important purpose by enabling us to prove that the inequalities, as we now see them, are far greater than can be accounted for upon the supposition of a solid globe contracting from cooling only.

Assuming then that a solid crust rests in corrugations upon a fluid layer, which is capable of yielding to such forces as the gravitation of the crust exerts, we have to consider the conditions of its equilibrium. Now the disturbances which we see that it has experienced, to the greatest depths which denudation exposes to observation, would lead us to suppose that, when once disturbed in lengths of any extent, it might perhaps be considered flexible. Moreover, if it rests in corrugations upon the subjacent fluid, it must be in unstable equilibrium; that is, the corrugations can have no particular relation to the places where they occur, but might exist equally well at some other. Here we should at once encounter a condition which would render oscillations of the surface possible, provided we could account for the disturbance of the position of equilibrium.

It will however be seen that the results of the following investigation are so remotely akin to natural appearances, that it must follow that the assumption of the flexibility of the crust is not true to fact. So that rigidity appears to play an important part in determining the position and form of the inequalities. This negative result is of itself of sufficient importance to make it worth while to give the investigation, irrespective of the intrinsic interest of the problem from a mathematical point of view.

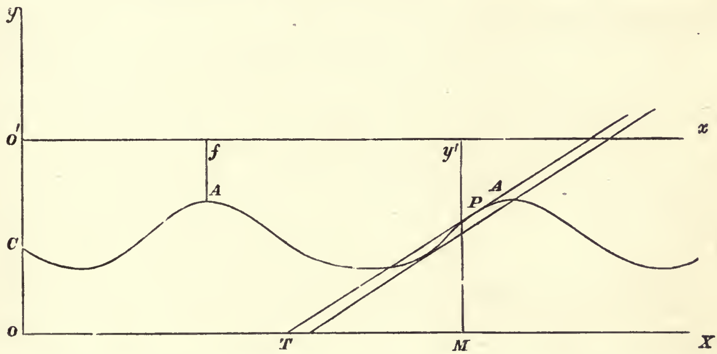
In order to obtain some insight into the general character of the corrugations, we will begin by enquiring what form would be assumed by a heavy flexible crust, resting upon a liquid within a rectangular trough shorter than the crust, for this would give an approximate idea of the contour of the surface upon the course of a section of the sphere perpendicular to its surface and cutting a set of corrugations at right angles.

It may be assumed that the trough and crust were originally of the same length, and that the corrugations have been produced by the ends of the trough having been made to approach each other.

Suppose

COX to be a vertical section of the trough,
CPA the section of the crust whose thickness is k,
OX, horizontal, the axis of x,
OY, vertical, that of y,
$OM = x$, $MP = y$, $PTM = \theta$,
r the radius of curvature at P,
ρ the density of the crust,
σ that of the liquid,
p the pressure of the liquid at P,
t the force of compression in the crust in the direction of the tangent at P.

We may suppose for the present that the flexibility of the crust is not impaired by its thickness.



Then applying the conditions of equilibrium we obtain the following equations:

(1) $t = g\rho k\,(C - y)$, where C is an arbitrary constant.

(2) $p = g\rho k \cos\theta - \dfrac{t}{r}$.

Let h be the value of the ordinate at the highest point A, and f the depth of a layer of fluid of the density of the crust, which upon a unit of area would produce a pressure equal to the compression at A. Let δ be the depth of a layer of fluid of the density of the crust, which would produce a pressure equal to the fluid-pressure at A.

Then from (1) we have

$$\text{compression at } A = g\rho kf = g\rho k\,(C-h);$$

$$\therefore\ f = C-h.$$

Also $p =$ the pressure due to the depth below $A +$ the pressure at A;

$$\therefore\ p = g\sigma\,(h-y) + g\rho\delta.$$

Therefore, substituting in (2),

$$g\sigma\,(h-y) + g\rho\delta = g\rho k\left(\cos\theta + \frac{y-C}{r}\right).$$

Change the origin to O' by putting $y' = y - C$;

$$\therefore\ y = y' + C = y' + h + f.$$

Hence when $y = h$, $y' = -f$, so that f is the depth of A below the new origin, and

$$h - y = -(y' + f);$$

whence, by substituting for y,

$$-g\sigma\,(y'+f) + g\rho\delta = g\rho k\left(\frac{dx}{ds} + \frac{y'}{r}\right),$$

or
$$-\sigma y' - (\sigma f - \rho\delta) = \rho k\left(\frac{dx}{ds} + \frac{y'}{r}\right).$$

Integrated this gives,

$$-\frac{\sigma y'^2}{2} - (\sigma f - \rho\delta)\,y' = \rho k y'\frac{dx}{ds} + D,$$

or
$$\rho k y'\frac{dx}{ds} + (\sigma f - \rho\delta)\,y' + \frac{\sigma}{2}\,y'^2 = -D \ldots\ldots\ldots\ldots\ldots\text{(A)}.$$

If this equation be solved with regard to y' it will give two values of y' for one value of $\dfrac{dx}{ds}$ or $\cos\theta$.

And if it be solved with regard to $\dfrac{dx}{ds}$, or $\cos\theta$, it will give the same value for $\cos\theta$ whenever y' is the same.

The curve is therefore of an undulating form as in the figure.

If the two values of y' be made equal, they will give the points of contrary flexure.

If we put $\frac{dx}{ds} = 1$, this will give the maximum and minimum ordinates.

Now we know that one of these is $-f$. Hence we may find D;

$$\therefore\ D = \sigma \frac{f^2}{2} + \rho (k - \delta) f.$$

Substituting this value we get for the said ordinates,

$$-f - \frac{\rho}{\sigma}(k - \delta) \pm \frac{\rho}{\sigma}(k - \delta),$$

and their difference,

$$2\frac{\rho}{\sigma}(k - \delta).$$

We have from equation (2), by making r infinite, since t is not infinite, at a point of contrary flexure,

$$p = g\rho k \cos\theta,$$

or

$$-g\sigma(y' + f) + g\rho\delta = g\rho k \cos\theta.$$

Substituting the corresponding value of $\cos\theta$ in the general equation, we get

$$y' = \pm\sqrt{f^2 + 2\frac{\rho}{\sigma}(k - \delta) f}$$

for the depth of the point of contrary flexure. The negative sign must be taken.

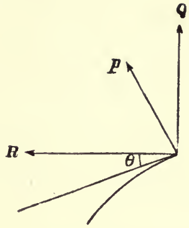
The radii of curvature are, at an anticlinal,

$$r = \frac{kf}{k - \delta},$$

and at a synclinal,

$$r = \frac{kf}{k - \delta} + 2\frac{\rho}{\sigma} k.$$

To determine the constant f we must discuss the conditions at an anticlinal. Consider any element ds of the curve, and let the horizontal and vertical components which act upon it from beyond at one extremity of it be



R and Q, and, as before,

p the fluid pressure,

t the compression,

θ the inclination of the tangent to the horizon.

Resolving vertically, we have for equilibrium,

$$p \cos \theta ds + t \sin \theta - g\rho k ds + Q = 0,$$

and horizontally,

$$p \sin \theta ds - t \cos \theta + R = 0.$$

Eliminating t,

$$pds - g\rho k \cos \theta ds + Q \cos \theta + R \sin \theta = 0 \ldots\ldots\ldots (1).$$

Now this being true when ds is indefinitely diminished, since Q and R are not thereby altered, we have generally,

$$Q \cos \theta + R \sin \theta = 0.$$

But at an anticlinal Q and R must be the vertical and horizontal components of the compression at one side of the anticlinal. From the nature of the case the vertical components of the compression on either side of the anticlinal must act in the same direction; and therefore they cannot be in equilibrium unless they are separately zero. And Q is one of them;

$$\therefore \; Q = 0;$$

$$\therefore \text{ also } R \sin \theta = 0,$$

whence either $\sin \theta = 0$, or $R = 0$.

Let us consider these two cases in order. When the element is under the action of the forces Q and R at one extremity of it, and ds is indefinitely diminished, observing that

$$Q \cos \theta + R \sin \theta = 0,$$

we have from (1) in the limit

$$pds - g\rho k \cos \theta ds = 0,$$

$$\therefore \; \frac{pds}{grk \cos \theta ds} = 1.$$

Now at an anticlinal we know that $p = g\rho\delta$, and (in the case we are now considering) $\cos\theta = 1$, and these are the values which these quantities assume there when ds is indefinitely diminished.

Hence at an anticlinal the above becomes in the limit

$$\frac{g\rho\delta}{g\rho k} = 1,$$

that is, when the curve is horizontal at an anticlinal,

$$\delta = k.$$

This condition evidently belongs to the case of a crust lying horizontally upon the liquid, which is a particular case of the general problem. In this instance the difference of the maximum and minimum ordinates $\frac{2\rho}{\sigma}(k-\delta)$ as found p. 103 vanishes, as it ought to do.

Next consider the second case, viz. when $R = 0$. In this case $f = 0$ since f measures R.

The relation $f = 0$ shows that there is no compression at the anticlinal.

In this case $\tan\theta = -\frac{Q}{R}$ takes the form of $\frac{0}{0}$.

The origin of co-ordinates is by the same relation for f brought to the level of the anticlinals.

Our equation (A) now becomes, suppressing the dash over the y,

$$\rho k y \frac{dx}{ds} - \rho\delta y + \frac{\sigma y^2}{2} = -D\,;$$

$$\therefore \frac{dx}{ds} = \frac{\rho\delta y - \frac{\sigma y^2}{2} - D}{\rho k y}.$$

This expression shows that $D = 0$, otherwise $\frac{dx}{ds}$ (or $\cos\theta$) would be infinite at an anticlinal where, reckoning from the newly-determined origin, $y = -f = 0$.

Dividing the numerator and denominator by y,

$$\frac{dx}{ds} = \frac{\rho\delta - \sigma\frac{y}{2}}{\rho k}.$$

Integrating this equation, and taking the origin at an anticlinal, we obtain

$$x^2 + y^2 - \frac{4\rho}{\sigma} x \sqrt{k^2 - \delta^2} - \frac{4\rho}{\sigma} \delta y = 0.$$

This represents a circular arc.

If λ be the distance from one anticlinal to the next, we obtain for δ the value

$$\delta = \sqrt{k^2 - \frac{\sigma^2}{16\rho^2}\lambda^2},$$

and for the final equation

$$\left(x - \frac{\lambda}{2}\right)^2 + \left(y - \frac{1}{2}\sqrt{\frac{16\rho^2}{\sigma^2}k^2 - \lambda^2}\right)^2 = 4\frac{\rho^2}{\sigma^2}k^2;$$

which gives the co-ordinates of the centre

$$\frac{\lambda}{2}, \text{ and } \frac{1}{2}\sqrt{\frac{16\rho^2}{\sigma^2}k^2 - \lambda^2};$$

and the radius $\dfrac{2\rho}{\sigma}k$.

These conditions are possible so long as λ is less than $\dfrac{4\rho}{\sigma}k$; when it has that value the festoon between the anticlinals becomes a semicircle.

It appears therefore that, when no extraneous force acts upon the crust, a section of it will assume the form of a series of equal circular arcs, the radii of which depend solely upon the mass (ρk) of a unit of length of the crust, and upon the density σ of the liquid on which it is supported. The degradation of the undulating curve into a series of circular arcs takes place through the compression at the anticlinal f becoming zero. The curvature (p. 103) at an anticlinal in this case becomes infinitely great.

The next step is to conceive a long trough, bent lengthwise into a circular form of large radius, and that the crust and liquid are acted upon by a force gravitating towards the centre, and so we approximate to what would be, in the case we have supposed, a section of the earth's surface, taken along a great circle. In this case we must suppose the curve reentering. No extraneous force acting, the circular festooned form will approximately represent the curve of equilibrium.

But for simplicity of conception it will be better to return to the idea of the rectangular trough, and to suppose that the curve is in the phase of an anticlinal where it meets the trough at either end; which condition is necessary for equilibrium if there is no friction there: and that there are m festoons.

Having found the form of equilibrium we have now to enquire what relation it holds to the length of the trough.

We will consider that the trough was originally of the length L, and that it is compressed until its length becomes $L(1-\epsilon)$. Then since $\frac{2\rho}{\sigma}k$ is the radius of every circular arc which can admit of equilibrium, and that the chords of the m festoons, or arcs, must equal the reduced length of the trough, putting ϕ for the angle subtended by each festoon, we must have

$$m \,.\, 2 \,.\, \frac{2\rho}{\sigma} k \sin\frac{\phi}{2} = L(1-\epsilon),$$

and because the whole length of the crust is that of the trough before compression,

$$\therefore \quad m \frac{2\rho}{\sigma} k\phi = L;$$

whence

$$\frac{\sin\frac{\phi}{2}}{\frac{\phi}{2}} = 1-\epsilon \qquad\qquad (1),$$

and

$$m = \frac{\sigma}{2\rho}\,\frac{L}{k\phi} \qquad\qquad (2).$$

Any value of ϵ less than unity substituted in (1) will give a corresponding value of ϕ, and thence from (2) a value of m.

But none but integral values of m will be compatible with equilibrium, because the curve must meet either end of the trough at an anticlinal.

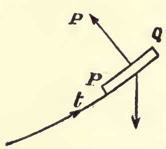
ϕ diminishes as m increases, and when m is infinitely great and ϕ infinitely small,

$$\frac{\sin\frac{\phi}{2}}{\frac{\phi}{2}} = 1 - \epsilon$$

becomes unity, and therefore $\epsilon = 0$, or there is no compression.

m also increases as k diminishes. Hence, *cæteris paribus*, the festoons are more numerous with a thin crust than with a thick one. The geometrical relations show that the lengths of the festoons must increase, and their number diminish, as the compression is increased.

Consider now the crust to be in equilibrium with a certain pair of values of m and ϵ, m being integral, and let the trough then be shortened, but not sufficiently so for the next integral value of m to be reached. What will happen? The festoons cannot alter their curvature so as to adapt themselves to their lessened chords. It seems then that the compression at the anticlinals must become finite, instead of being zero, and that the ends of the festoons will be pushed up against each other, by which means material will be accumulated there, and Q and R will become finite at the anticlinals. This will accord with the undulating form of the curve, but it seems that the equilibrium must become unstable, on account of the weight resting on the anticlinal in the highest possible position. It will therefore be liable to slide down upon the surface of the liquid on one side or other of the anticlinal.



Let us suppose then that a portion of the crust PQ, whose weight is Q, has become thickened by the accumulation of material, and let P be the mean fluid-pressure upon this portion of crust. And let it be supposed that the curve is cut off above Q.

Then we have for the equilibrium of PQ,

$$Q \sin \theta = t$$

and $$Q \cos \theta = P \times PQ.$$

The first of these equations combined with (1) p. 101, and with the equation to the circle, will give the position of the point P to which PQ will descend, balancing the curve below it in the same manner as it would be balanced if the curve were continuous above instead.

The fluid-pressure need not be altered by the above supposition, because the general pressure will keep the anticlinal filled, although some of it should tend to escape above PQ. But if the crust which was upon the other side of the anticlinal, surmounts it, and follows PQ down the incline, it will produce compression above Q, and it seems that Q must, in that case, descend to the synclinal, where if ρ be less than σ it will float and not disturb the general form of the curve beyond it. PQ, while thus descending, will force up the crust on the other side of the festoon, which in its turn surmounting the anticlinal, will descend the next incline. These changes, however, will take place rather by a relative movement of the liquid, than by an absolute movement of the crust, and will cause alternate elevation and depression of any given point in the crust.

If the conditions of perfect fluidity in the liquid and perfect flexibility in the crust were fulfilled, and the crust were of insensible thickness, then our result would be true, and the festoons be all equal circular arcs. But under such circumstances as may be supposed to exist in nature, these conditions would be so imperfectly fulfilled, that all we can assert is that the above considerations will serve as a rough guide to what we might expect to occur. The conditions of equilibrium would be satisfied from point to point not very widely distant from each other, and we might expect the compression to be distributed unequally. Hence the festoons would be larger, and the anticlinals higher, in some regions than in others. The local thickenings of the crust would also render the forms of the curve locally to differ from the normal form. The effect

of the hydrostatic pressure of the oceans must also be taken account of.

For suppose that any portion of the curve be covered by a heavy fluid (the ocean) of density μ. Let c be the height of this ocean above the level, upon which the origin was taken in the first instance (on p. 101). Then, in forming the equations of equilibrium, we shall have as before

$$t = g\rho k\,(C - y).$$

But for p we shall have,

$$\begin{aligned} p &= \text{pressure at } A + \text{pressure due to the depth below } A \\ &\quad - \text{pressure due to the ocean}, \\ &= g\sigma\,(h - y) + g\rho\delta - g\mu\,(c - y), \\ &= -g\,(\sigma - \mu)\,y + g\,(\sigma h - \mu c) + g\rho\delta. \end{aligned}$$

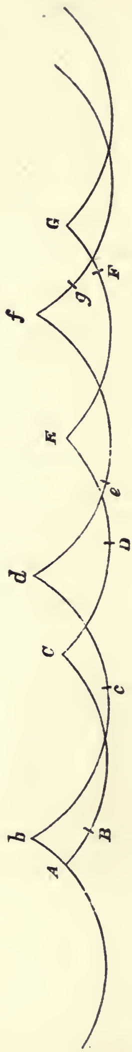
This is an equation of the same form as before. We cannot find the values of the constants exactly as we did before, but we can see that the curve must consist of two portions, one above the ocean and one below it.

At the point where these two portions meet one another, the direction of the curve must be continuous. The portion above this point will evidently follow the law already investigated in all respects. For the part below we have expressions for p and t of the same form as before. This part will, therefore, follow the law of a curve, which may be supposed to rest upon a liquid of the density $\sigma - \mu$ instead of σ. The law of form will, therefore, be the same as before, the constants being determined according to this supposititious case.

The *compression* (t) of the part of the curve beneath the ocean will, however, be greater than in the supposititious case, the crest being higher. But that will not alter its *form*, because (t) in any case varies according to the height of the anticlinal. The radius of the portion beneath the ocean will be

$$2\,\frac{\rho}{\sigma - \mu}\,k.$$

The diagram illustrates the effect of increased general compression in elevating and depressing points in the crust. The



point A is supposed for simplicity to remain fixed. Then the points B, C, D, E, F, G, upon further compression assume the positions b, c, d, e, f, g.

If we now pass to the case of any rectangular vessel, and suppose it to be compressed in two orthogonal directions, then the form of the corrugations in these two directions would be approximately governed by the laws we have investigated, provided the plane crust be supposed capable of adapting itself to a form not developable into a plane. And even in the case of a contracting sphere, the *character* of the corrugations may be assumed to be on the whole similar to that investigated, and they would be arranged about polygonal areas.

The consequence of the crust not being absolutely flexible would be, that it would rest within certain limits in forms either more or less curved than the proper surface of equilibrium, and it is obvious that the considerable ratio, which the thickness of the crust may bear to the radius of the festoon, renders the result of the investigation much more difficult of application to the case of nature than it would otherwise be. But we may use it for what it is worth.

Still further, the general compression along a great circle will very inadequately represent what would occur when the compression over the whole surface has to be taken into account. In short, the case of nature is so extremely complicated, that it is very difficult to reason satisfactorily upon it. Nevertheless the conclusions we have arrived at concerning the case of a thin heavy flexible crust resting on a perfect liquid, have a certain value as far as the results may be summed up in the following propositions:

(1) A vertical section of the lower surface of the crust, where it meets the liquid, when carried across a series of the corrugations, would present a series of festoon-like arcs approximating more or less to a circular form, concave upwards.

(2) The anticlinals would be of cusp-like form, resulting from the intersection of contiguous festoons. There would not be found anything of the character of rolling undulations with flat-crested anticlinals.

(3) The degree of curvature of the festoons would not depend upon the amount of the general compression, though their amplitude, or the distance from crest to crest, would be increased, under circumstances of equilibrium (*i.e.* where m, p. 107, is integral), upon the compression being increased. The curvature would diminish upon the thickness being increased because $r = 2\frac{\rho}{\sigma}k$.

(4) But the curve of equilibrium, even if it were once exactly established, could not be strictly maintained during successive contractions of the sphere, because we have seen that, in the case of a trough of given length, a flexible crust and a perfect liquid, equilibrium can only subsist for certain special amounts of contraction having relation to the length of the trough. No doubt the limits within which this would be possible would be enlarged by defects in flexibility and liquidity, and they would also be enlarged by a great increase in the length of the trough as compared to the thickness of the crust: for then m would always be nearly an integer.

It can scarcely admit of a doubt that the results we have just obtained do not represent the case of nature. There is no reason to believe that the crust of the earth is flexed on the whole in such a manner, that a section carried across a series of anticlinals would give a series of circular arcs of the dimensions required by our results; and accordingly our assumptions cannot be in accordance with the facts. The radius $\frac{2\rho}{\sigma}k$, σ being necessarily greater than ρ, is too small to suit the arrangement of the chains.

Now a negative conclusion of this kind is far from being without importance; for it is by a method of exhaustion that we are best able to attack our problem. What then are the assumptions upon which we have arrived at this unsatisfactory result? They are, that the crust is (1) thin, and (2) flexible, and (3) that it rests on a liquid substratum. Of these assumptions the first cannot be wrong. The crust is no doubt thin as compared with the other dimensions with which we have to deal; as, for instance, the width of a continent. That it rests on a fluid substratum has been already shown to be almost certain. It remains then that the error in our assumptions lies in supposing the crust flexible.

It follows that, when a horizontal pressure acts upon a tract of crust, we must not suppose that the yielding will take place by the bending of the crust, while its uniform thickness is maintained; for, if that were the case, an arrangement of anticlinals, bearing a close resemblance to the result of our investigation, would be produced. But we must rather suppose that the yielding takes place through a crushing together and thickening of the crust.

The consequence will be that elevations above the datum level will be accompanied by depressions beneath. The anticlinals will not be filled with fluid from below, but will be the upper portions of double bulges, which will dip into the fluid below as well as rise into the air above. And this is the condition of things with which we shall attempt to deal in the following chapter.

CHAPTER X.

DISTURBED TRACT.

Conditions of equilibrium of a disturbed portion of the earth's crust—Probable result of compression—Part of the crust sheared upwards and part downwards—These separated by a "neutral zone"—Calculation of its position—Confused contortions in highly metamorphosed rocks explained—Depth of neutral zone—Probable thickness of crust—Prof. A. Favre's experiments on contortions—Inverted flexures—Formation of a mountain chain and its elevation above the ocean—Subsequent effects of denudation and accumulation of additional sediment—Consequent tilting of tract—Also general elevation from the same cause—Why areas of deposition are sinking areas—Degree of tilting depends upon width of tract and the existence of a mountain chain.

In this chapter we shall consider the process of elevation of a mountain chain by horizontal compression under the condition of a certain degree of rigidity in the crust, without making any particular assumption regarding the cause of compression, assuming only that it acts in a horizontal direction, and that the crust rests on a fluid substratum of greater density than its own. Under these circumstances, if a hole were made anywhere in the crust, the fluid would rise in it to a certain level, which we may call the effective level of the fluid.

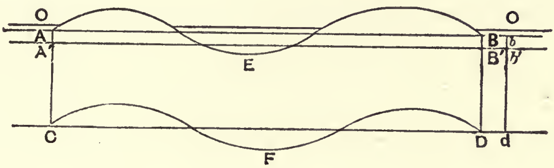
Let us as before call the density of the crust ρ, and that of the fluid σ, σ being greater than ρ. And let our sections be in all cases supposed to be of unit of width.

In the figure let $A'B'b'$ be the effective surface of the fluid, the crust beyond AC and BD being undisturbed by compression. Then by the principles of hydrostatics

$$B'D = \frac{\rho}{\sigma} k.$$

And $$BB' = \frac{\sigma - \rho}{\sigma} k.$$

But if we suppose an ocean OO, of depth δ and density μ, to



cover the crust, we must add the pressure of the ocean to that of the crust and then

$$B'D = \frac{\rho k + \mu\delta}{\sigma}.$$

And $$BB' = \frac{\sigma - \rho}{\sigma} k - \frac{\mu}{\sigma} \delta.$$

To avoid circumlocution, we shall in future call the portion of the crust AB, which has been either partially or generally compressed, and disturbed from its original position between AC and BD, "the tract."

We shall also make a fresh supposition about the levels, above and below which $\Sigma(a)$ &c. are measured; no longer regarding them as what would have been the upper and under surface of the crust if it had been perfectly compressible in a horizontal direction; but now, as "mean levels," above and below which the tract is disturbed as compared with undisturbed regions. The consequence of this alteration will be, that when a volume β is depressed into the fluid, it will not follow that an equal volume α will rise into the anticlinals, because the positions of the mean levels themselves will be affected. Hence $\Sigma(\alpha)$ will not be equal to $\Sigma(\beta)$, and we cannot say that $\Sigma(a) - \Sigma(b)$ is equal to *kle.*

Supposing no attachment at AC and BD, if there be equilibrium of the whole as a rigid mass, we must have two conditions fulfilled, (1) that the weight of the fluid displaced equals the weight of the whole mass, and (2) that the centre of gravity of the whole mass is vertically above that of the fluid displaced.

Let us consider the former condition first. Then, using the same letters, but dashed, to refer to the effective surface of the fluid, as we have used for the upper surface of the crust, p. 46, and supposing that a volume $\Sigma(d)$ of water covers the hollows of the disturbed region, since the weight of the fluid displaced is

$$g\sigma A'EB'DFC, \text{ or } g\sigma\{kl(1+e)-\Sigma(a')+\Sigma(b')\},$$

and the weight of the mass supported is

$$g\rho kl(1+e)+g\mu\Sigma(d),$$

we have for the first condition of equilibrium,

$$\sigma\{kl(1+e)-\Sigma(a')+\Sigma(b')\}=\rho kl(1+e)+\mu\Sigma(d).$$

$$\therefore\ \Sigma(a')-\Sigma(b')=\frac{\sigma-\rho}{\sigma}kl(1+e)-\frac{\mu}{\sigma}\Sigma(d).$$

But we also have the geometrical relation,

$$\begin{aligned}\Sigma(a')-\Sigma(a)+\Sigma(b)-\Sigma(b') &= \text{rectangle } AA'B'B,\\ &= BB'\times l,\\ &= \frac{\sigma-\rho}{\sigma}kl-\frac{\mu}{\sigma}\delta l,\end{aligned}$$

therefore subtracting,

$$\Sigma(a)-\Sigma(b)=\frac{\sigma-\rho}{\sigma}kle+\frac{\mu}{\sigma}(\delta l-\Sigma(d)).$$

Since δl is the volume of water which would have covered the tract if there had been no elevations, and $\Sigma(d)$ is the volume of the water which rests upon it after elevation, we may in general take $\delta l-\Sigma(d)$ to be the volume of rock elevated so as to replace water below the water level. If there be no compression, or $e=0$, then $\Sigma(d)$ is δl; so also if $\sigma=\rho$, for in that case the crust would float in the fluid without any part emerged, so that $\Sigma(d)$ would be δl. Thus either of the conditions $e=0$, or $\sigma=\rho$, should reduce each side of our equation to zero, as it will evidently do.

Since

$$\Sigma(a) - \Sigma(b) + \Sigma(\beta) - \Sigma(\alpha) = kle,$$

$$\therefore\ \Sigma(\beta) - \Sigma(\alpha) = kle - \frac{\sigma - \rho}{\sigma} kle + \frac{\mu}{\sigma} \Sigma(d) - \frac{\mu}{\sigma} \delta l,$$

$$= \frac{\rho}{\sigma} kle - \frac{\mu}{\sigma} (\delta l - \Sigma(d)).$$

Hence

$$\Sigma(\beta) - \Sigma(\alpha) - \{\Sigma(a) - \Sigma(b)\} = kle - 2\frac{\mu}{\sigma} (\delta l - \Sigma(d)).$$

Now $\delta l - \Sigma(d)$, is evidently the volume of the water which has been displaced from the elevated part of the crust by its being elevated. And if we compare the densities of water and rock, it is evident that $\frac{\mu}{\sigma}$ is less than 2, and therefore $2\frac{\mu}{\sigma}(\delta l - \Sigma(d))$ less than the volume of water which has been displaced. But the latter is obviously less than the entire volume which has been added to the part of the crust between A and B by compression, that is less than kle. This shows that $\Sigma(\beta) - \Sigma(\alpha)$ is always greater than $\Sigma(a) - \Sigma(b)$, but that this excess is diminished by an ocean, so that it will increase the amount of elevation of the surface above the upper mean level.

If we neglect the effect of the ocean, because $\frac{\mu}{\sigma}$ is small and δ much less than k, then

$$\frac{\Sigma(\beta) - \Sigma(\alpha)}{\Sigma(a) - \Sigma(b)} = \frac{\frac{\rho}{\sigma} kle}{\frac{\sigma - \rho}{\sigma} kle} = \frac{\rho}{\sigma - \rho}.$$

And consequently, if σ were infinite we should have

$$\Sigma(\beta) - \Sigma(\alpha) = 0,$$

as would be the case, for under such circumstances the crust could not dip down into the fluid, nor the fluid rise into anticlinals. If we suppose the crust to be of the density of granite,

viz. 2·68, and the subjacent fluid of the density of basalt, viz. 2·96,[1] we have,

$$\frac{\Sigma(a) - \Sigma(b)}{\Sigma(\beta) - \Sigma(\alpha)} = \frac{\sigma - \rho}{\rho} = 0{\cdot}104,$$

or about $\frac{1}{10}$. The diminution of density by elevation of temperature need not be regarded[2].

We have thus far had under consideration the relations of the elevations and depressions above and below the mean levels, as they would depend upon the conditions of equilibrium of a mass of any form, floating without external constraint upon a fluid, so far as the vertical forces are concerned. We must however also consider how much importance belongs to the attachments of the tract at its extremities *AC* and *BD*; and on this account, and from our general ignorance of the amount of rigidity which must be attributed to the mass, it is obvious that we can only arrive at the most general conclusions.

Let us then suppose as before, that the portion of crust *AbdC* is compressed into the position *ABDC*. The lateral pressure will act in some straight line drawn from a point in *AC* to a corresponding point in *BD*. Consequently it will have no tendency to open any fissures in the crust. We may therefore dismiss from consideration such a condition of things as is represented in our second figure of the diagram, p. 46.

If the crust were flexible and elastic, such a form as that represented in the first figure might be possible. But the condition more in accordance with natural appearances is that it is partially plastic and partially brittle. It is capable of withstanding considerable pressure until it gives way, but when it does so, the harder portions will break up, and the plastic accommodate themselves among the interstices of the harder.

[1] Calculating from Laplace's law of density,

$$\frac{d\rho}{dr} \text{ at the surface} = 0{\cdot}00000053,$$

r being expressed in feet. Consequently the increase to ρ in 20 miles would be 0·056 and the density of Basalt would be met with at 100 miles. But Laplace's law is empirical, and does not give information as to the actual arrangement of the successive couches.

[2] Note, p. 68.

Under these circumstances we may suppose that, upon the pressure increasing at any place, the crust will give way first at some place where it is weakest. It cannot give way at two places at once. Once having given way, the weak condition will be propagated from that place, but more in one direction than the opposite. In which direction this will be, will depend upon the nature of the first rupture; for it is not likely that it should be perfectly symmetrical: and if not so, this will render the neighbouring crust weaker on one side than on the other, and in that direction we may expect the disintegrating process to extend. The pressure approximately horizontal will produce a shear more or less vertical. It will become a question therefore whether the compression is to be compensated by great elevation above, and great depression below the mean levels for a short horizontal space, or by lesser elevation and depression for a longer. But the resistance to be overcome in shearing will increase as the thickening increases, for not only will a higher column of rock then have to be pushed up against gravity above the upper mean level, but also the resistance to depression below the lower mean will be increased on account of the increased upward pressure of the fluid down into which the lighter rock is forced. The friction will be increased also. There will therefore arise some relation between the amount of vertical shear, and the strength of the crust. In other words, $\Sigma(a) - \Sigma(b) + \Sigma(\beta) - \Sigma(\alpha)$ being equal to kle, the length l of crust, broken up to give the necessary relief by the formation of the inequalities represented by these symbols, will depend upon the strength of the crust; such length being greater or less according as the crust is weaker or stronger, because, if strong, it will resist further fracture, and the part already disturbed will be utilised to satisfy any subsequent additional compression.

The horizontal force causing a compression which as already remarked will be relieved rather by crushing or corrugating the rocks than by flexing the crust as a whole, will not as a primary consequence produce the hollows belonging to $\Sigma(b)$ and $\Sigma(\alpha)$. It will rather result in a swelling of the crust upwards and downwards.

The experiments which M. Tresca made upon the flowing of plastic, and even very hard, substances have a bearing upon this subject. His paper has the appropriately paradoxical title, "De l'écoulement des corps solides[1]." In it he showed that solid, ductile, soft, or pulverulent, bodies can, without changing their state, flow in a manner analogous to that of liquids, when sufficiently great pressure is exerted on their surface. The substances were subjected to pressure confined in a rigid cylindrical envelope, pierced at the bottom with a concentric orifice of varied dimensions. These experiments would lead us to expect, that the materials of the earth's crust, when laterally compressed by a sufficient force, would "flow," or be strained, vertically. What would be analogous to the stream lines in a liquid would represent the distortions of the rocks; thus might be produced cleavage, foliation, or contortion, according to the conditions of the rocks themselves, or of the neighbouring masses which adjoined them[2].

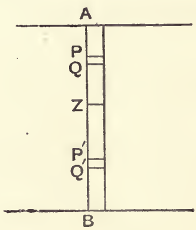
But although we cannot predict the exact manner in which the material will be arranged which constitutes the protuberances that will be produced above and below the mean levels of the crust, yet we may arrive at some conclusion as to whether the material at any given level within the crust will on the average be sheared upwards or downwards by the compression. For this compression acting horizontally, will have no tendency in itself to determine whether the motion which it causes in an elementary portion of the crust subjected to its action shall be upwards or downwards. That

[1] *Comptes rendus*, 1865, Vol. LX. p. 1228.

[2] M. Tresca's experiments appear to have led to the misconception, that great pressure causes solid substances to become liquid. A writer under the initials E. L. C. has said that, if the earth were of steel, at the depth of one or two leagues it would be of flowing steel, and that we should have to decide whether flowing steel was solid or fluid (*Eng. Mechanic*, Vol. XII. p. 254). But in M. Tresca's experiments, one surface of the substance was subjected to enormous pressure, while the orifice from which it flowed was subject to none at all. This is quite a different case from that of the interior of the earth, where the pressure is alike in all directions. So far from great pressure rendering the interior fluid, it appears from the proved great rigidity of the earth as a whole, that it has an opposite effect.

circumstance will therefore be determined by the resistances which operate to resist the motion of the element in question. These will be the weight of the element acting downwards, the friction tending to resist the motion, and opposite to it in direction, and the pressure of the crust upon the fluid beneath. It is evident that the friction among the particles of the crust will depend upon its physical state. If it were rigid it would be infinite; if liquid it would be zero. As it is, we may conclude that its plasticity is increased in the lower parts by the influence of the high temperature there. We will therefore assume the friction to be some function of the depth. It will be sufficient for our purpose to consider the density constant throughout the thickness of the crust.

Let then $AB = k$ be the thickness of the crust before motion commences. When the lateral pressure has become so great



that compression begins to take place, suppose that the portion AZ of the column AB is sheared upwards, and the portion ZB downwards, and let us call the locus of the point Z the "neutral zone."

Let p be the force at P which resists the upward motion of the element PQ;

$$AP = x, \quad PQ = dx,$$

$f =$ the frictional force, which we assume to be a function of x.

Then $$dp = g\rho dx + f dx;$$

$$\therefore \quad p = g\rho x + \int f dx + C.$$

At A, $x=0$; and f has some constant value; and there is no extrinsic pressure at A;

$$\therefore \quad p = g\rho x + \int_0^x f dx \text{......................(1).}$$

Again, if p' be the force at P' which resists the downward motion of ZP',

$$dp' = -g\rho\, dx + f dx;$$

$$\therefore \quad p' = -g\rho x + \int f dx + C' \text{..............(2).}$$

Now, at the neutral zone, the forces which resist the upward motion must be equal to those which resist the downward motion. If then h be the depth of the zone, the integral (1), taken from 0 to h, must be equal to (2) taken from h to k, when to the second we have added the extrinsic force $g\rho k$, arising from the pressure of the fluid;

$$\therefore \quad g\rho h + \int_0^h f dx = -g\rho\,(k-h) + \int_h^k f dx + g\rho k;$$

$$\therefore \int_0^h f dx = \int_h^k f dx.$$

This result shows that the position of the neutral zone depends solely upon the condition, that the friction upon the column above it shall be equal to that upon the column below it.

Let us first of all make the assumption that the friction is constant at all depths, and equal to λ.

Then $$\lambda h = \lambda\,(k-h);$$

$$\therefore \quad h = \frac{k}{2},$$

or the neutral zone is situated in the middle region of the crust.

But if we consider the plasticity of the material to be increased by heat in the lower parts of the crust, we must

assume some function to express the value of f which has a constant value λ at the surface, and decreases, at first slowly, and afterwards more rapidly, as the depth x is increased.

For the purpose in hand we may admit that it becomes zero at the bottom of the crust, although this is of course not strictly true. An appropriate function for our purpose appears to be,

$$\lambda \cos \frac{\pi x}{2k}.$$

The corresponding force upon an element at P will therefore be $\lambda \cos \frac{\pi x}{2k} dx$; and the whole frictional force upon AZ will be

$$\int_0^h \lambda \cos \frac{\pi x}{2k}\, dx,$$

or,

$$\lambda \frac{2k}{\pi} \sin \frac{\pi h}{2k}.$$

If we make $h = k$, we find the friction across the whole crust to be $\lambda \frac{2k}{\pi}$. And it is evident that the friction along AZ, *plus* the friction along ZB, make up the whole friction across AB.

Hence the friction along $ZB = \lambda \frac{2k}{\pi}\left(1 - \sin \frac{\pi h}{2k}\right)$.

Our condition for the position of the neutral zone then gives us,

$$\lambda \frac{2k}{\pi} \sin \frac{\pi h}{2k} = \lambda \frac{2k}{\pi}\left(1 - \sin \frac{\pi h}{2k}\right),$$

$$\text{or} \quad \sin \frac{\pi h}{2k} = \frac{1}{2},$$

$$\therefore \frac{\pi h}{2k} = \frac{1}{3}\frac{\pi}{2};$$

$$\therefore h = \frac{1}{3}k.$$

In this case then the neutral zone would be at one-third of the depth.

In finding the position of the neutral zone no assumption has been made respecting the value of k. So long as the crust at the place under consideration was not under constraint, but in equilibrium as a floating body, the neutral zone would hold the same relative position within it. So that if this condition of flotation remained fulfilled it would be always horizontal. For the purpose we have in view it will be sufficiently accurate to assume, that it occupies a position whose distances from the upper and lower surfaces of the crust, whether before or after compression, are always in the same ratio.

It is also observable that the position of this zone does not depend upon the absolute value of the frictional force, so long as it can be expressed as a function of the depth multiplied by the value which the friction has at the surface. If however the friction were to vanish, which it would do if the material were to become liquid, we should have h indeterminate. The signification of this appears to be, that the tendency of the liquid at every depth would then be indifferently to move upwards or downwards, and therefore it would be affected with confused internal currents, somewhat similar to convection currents.

Now it will be observed that, since the shear, whether upwards or downwards, increases continually as we get further from the neutral zone, it follows that if there were to be a relative motion, in the opposite direction to that which they actually have, impressed upon the particles at any given level, that level would become a neutral zone relatively to the particles above or below it. If then the material above or below any zone were to become liquid, it would behave under this relation as the whole crust would behave if it were liquid, and would be affected with confused currents. This consideration goes far to explain the confused contortions constantly observable in highly metamorphosed rocks; which are supposed to have been softened by heat, and consequently to have approached the condition of a liquid.

The function $\lambda \frac{\cos \pi x}{2k}$, which we have provisionally adopted to express the friction at the depth x, would actually vanish at the bottom of the crust. This of course represents what could not really happen. It is however evident, that any softening of the lower parts would tend to raise the neutral zone to a higher position, and that the more complete the softening the higher it would be. We have here assumed an amount of softening which is too great, and find that the corresponding position of the neutral zone would be at the depth of $\frac{1}{3}$ of the crust. On the other hand we have found that, if the crust were equally rigid throughout, it would be situated at the depth of $\frac{1}{2}$ of the thickness. Comparing the two cases, we may fairly conclude that it will be in fact in a position intermediate between these two, and shall be probably justified in placing it at about $\frac{2}{5}$ of the thickness.

On different, and geological, grounds we have likewise the following considerations to help to guide us in affixing a limit below which we must place the neutral zone. It is well known that granite frequently occupies the central axis of mountain chains. Now Mr Sorby has taught us in his sectional address at the British Association, 1880,[1] that granite has been usually formed in the presence of liquid water; although he speaks of a single instance in which the temperature at the commencement of crystallization may have been higher than that at which water can exist in the state of a liquid. The critical temperature for water is about 773° F. At the rate of increase of one degree F. for 51 feet, this temperature would be reached at the depth of about seven miles and at the rate of one degree for 60 feet at the depth of about eight and a half miles. We may therefore conclude, that rock, which was once at seven or eight miles' depth, has been forced upwards, and consequently, that the neutral zone is lower than that.

But we know of no rocks at the surface except the truly eruptive, which have come from a greater depth than has granite. Had it come from the neutral zone, this would have made $\frac{2}{5}k$ equal from 7 to $8\frac{1}{2}$ miles, and therefore k would have

[1] *Nature*, Vol. XXII. p. 392.

been from 17 to 21 miles. But since it is very improbable that any portion of the neutral zone should be brought so near the surface as to be exposed, we may conclude that the crust is thicker than when thus estimated.

Again, Mr Mallet has determined the temperature of melting slag to be about 3000° F.[1], and we can hardly suppose that the materials of the crust could be solid at such a temperature, as can melt silicates without the presence of water. This temperature would, at the same rates of increase, be found at the depths of about 28 or 30 miles, consequently we may conclude from this argument that the crust is certainly less than 30 miles thick. One of these reasons then leads us to think that it is more than 17 or 21, and the other that it is less than 28 or 30 miles in thickness, according as we assume the higher or lower rate: and we have given reasons for preferring the higher[2]. Perhaps we shall not be very wrong in assuming 25 miles as its average thickness between the upper and lower mean levels.

In equation (1) p is the whole force which resists motion upwards, and at the neutral zone it is equal to

$$g\rho h + \int_0^h f\,dx;$$

which consists of two parts, namely the weight of the column of rock above that zone and the frictional force above it.

Similarly p' in equation (2), taken between the neutral zone and the bottom of the crust, gives the whole force which resists motion downwards, and this is found to be

$$g\rho h + \int_h^k f dx.$$

The first term of this expression is the same as in the previous one, and the second term is the frictional force below the neutral zone. By the property of the neutral zone these second terms are equal, and will evidently increase with the thickness of the crust. So also will the first term. And, since the depth h of the neutral zone has been shown to be proportional to the thickness of the crust, it follows that the first term is proportional to that thickness, and the second possibly may be so.

[1] p. 68. [2] p. 16.

This however depends upon the constitution of the crust. It may therefore happen that $g\rho h + \int_0^h f dx$ may be greater for a small value of h when f is great, than for a considerably larger value of h when f is small, that is the strength of the crust may present a greater obstacle to shearing than its thickness. This confirms what has already been said at p. 119 respecting a relation between the amount of vertical shear and the strength of the crust.

By the favour of Messrs Macmillan and of the proprietors of *La Nature*, one of the woodcuts is exhibited overleaf representing the results of some interesting experiments made by Prof. A. Favre of Geneva. The following description is from the translation given of Prof. Favre's article in *Nature*, Vol. XIX. p. 103. "I placed the layer of clay employed in these experiments on a sheet of caoutchouc, tightly stretched, to which I made it adhere as much as possible; then I allowed the caoutchouc to resume its original dimensions. By its contraction the caoutchouc would act equally on all points of the lower part of the clay, and more or less on all the mass in the direction of the lateral thrust......."

"The arrangement of the apparatus is very simple. A sheet of India rubber 16 mm. in thickness, 12 cm. broad, and 40 cm. long, was stretched, in most of the experiments, to a length of 60 cm. This was covered with a layer of potter's clay in a pasty condition, the thickness of which varied, according to the experiments from 25 to 60 mm. It will be seen from the dimensions indicated that pressure would diminish the length of the band of clay by one-third. This pressure has been exerted on certain mountains of Savoy......."

"The strata, which appear to divide the masses of clay, and which are represented in the figures, are not really strata, but simply horizontal lines at the surface of the clay"—probably he means ruled along the side of the mass before the compression was allowed to act. After describing the varied appearances, he observes, "The strata are less strongly contorted in the lower parts than in the neighbourhood of the upper surface. They are disjoined in certain parts by fissures or caverns. They are traversed by clefts or faults inclined or vertical. All these deforma-

Figure copied from *Nature*, Vol. xix., p. 104, illustrating Professor A. Favre's experiment upon the compression of a layer of clay.

tions are the more varied, in that they are not similar on the opposite sides of the same band of clay."

It appears then that instead of producing contortion at the layer which adhered to the caoutchouc band, the compression was there relieved by mere thickening, and that contortion became more and more pronounced at greater distances from this layer. To this uncontorted layer our neutral zone would correspond. As the upper surface of the crust was approached, the contortion would become more marked, simple compression accompanied by upswelling characterising the disturbance of the strata nearer to the neutral zone. On the character of the disturbance below this we cannot safely speculate. But probably, on account of the continually increasing pressure, and the softening of the material by heat, the contortions would, as already explained[1], be of a more confused character than above it.

It must not however be concluded that, because Prof. Favre's experiments so happily illustrate the point in hand, it follows that the theory which he proposed to illustrate by them is the same as the one which is now advanced; for the object he had in view was to test the effect of secular cooling alone, in producing inequalities upon the earth's surface, without the hypothesis of a fluid substratum. We can however easily see, that such a theory would require the entire globe to be covered, like the caoutchouc band, by these corrugations; instead of their being arranged, as they are, in mountain chains, to say nothing of the other objections which have been already stated in opposition to that theory.

Prof. Favre's experiments may also be fairly adduced to show how hopeless it would be to predicate the exact form which contortions would assume. Scarcely any conditions can be conceived more favorable to uniformity of result than those which he devised; and yet it is evident at a glance how varied was the result obtained at various points of the longitudinal section. In no respect do the experiments illustrate the case of nature more strikingly than in this apparent complexity of the disturb-

[1] p. 124.

ances. Yet it is certain that these must have been governed by unerring laws resulting from a slight want of homogeneity in the material: and perhaps even, as the figures seem to suggest, from the local weakness caused by the horizontal lines, which had been traced upon the face of the clay before compression.

In the case of Prof. Favre's experiments the compression was uniform along the tract. In that which we are considering it would be otherwise. The pressure would increase until the crust gave way at one place. There it would be crushed upwards and downwards, above and below the neutral zone, until the thickening had attained a certain limit, which would depend upon the rigidity of the material, its weight, and perhaps on the angle of repose. When this limit had been attained, a fresh region would be involved in the process; and this would probably adjoin the former. But as the lateral force expended itself, the thickness disturbed would become less and less, and the disturbance would gradually die out towards that side from which the movement came.

Since the portion of the crust which lay within the limits of the original thickness would suffer greater compression than the raised and depressed portions above and below it, being more in the direct line of the pressure, we might expect that the flexures would hang over towards the side from which the movement came, thus causing inversions of bedding. This phenomenon is common on the flanks of mountain chains, and especially marked in the Appallachians. In the explanation usually given, the pressure has been supposed to have come from the side, away from which the flexures incline: so that if the flexures incline towards the west, the movement is supposed to have come from the east[1]. According to the explanation now proposed the case would be the exact opposite, and if the flexures hang over towards the west, the movement came from the west. Where we find inverted bedding on both sides of an anticlinal, or a fan-shaped structure, we infer that a

[1] Prof. Green's *Geology for Students*, p. 377, with a reference to *Silliman's Journal*, 1st Series, XLIX. 284.

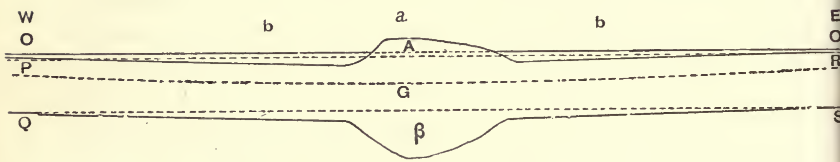
relative movement towards that region has occurred from the opposite sides.

We will now attempt to trace the formation of an elevated region by the compression of a tract of crust. We suppose that the compression has thickened the tract by crushing and corrugating the rocks of which it is composed, and that this thickening is greater about that part of the tract which was first compressed, and gradually subsides towards the region or regions from which the movement came. We also conclude that there will be what we have called a neutral zone, or level within the crust, above which the material will on the average be sheared upwards, and below it downwards. It will correspond with the caoutchouc band in Professor Favre's experiments. The position of the neutral zone will depend upon the law, according to which the plasticity of the material changes at different depths.

We have however seen that its position below the surface lies probably at between $\frac{1}{3}$ and $\frac{1}{2}$ of the total thickness of the crust, and that this result, combined with our knowledge of the temperature at which granite has been formed, shows that the total thickness must exceed twenty-one miles, and that this conclusion agrees well with the fact that the melting temperature of rock in the dry way may be expected to occur at a depth of less than about thirty miles.

We will take therefore as the bases of our working hypothesis, that the undisturbed crust is about twenty-five miles thick, and that the neutral zone is at the depth of about ten miles from the upper and fifteen from the lower mean level. Consequently, where the tract is compressed, it is easily seen that the thicknesses of the elevated and depressed portions above and below the mean levels will be in the same ratio, viz. of 2 : 3. Since the compression along the tract acts in a line which falls within the crust it will not have any tendency to open fissures, but on the contrary to prevent their being formed or to close them if they exist, so that the subjacent fluid will not as a rule gain access to, and overflow, the depressed parts $\Sigma(b)$ of the upper surface.

In the diagram suppose compression to have taken place by a movement of RS towards PQ. A portion of the crust will



Scale $\frac{1}{10}$ of an inch to 5 miles.
The upper line OO is the sea level.
The upper broken line is the upper mean level.
The middle broken line is the neutral zone.
The lower broken line is the lower mean level.
A the volume above the sea level.
β the volume below the lower mean level.
G is the centre of gravity of the tract.
The thickness of the crust is about 25 miles.
The length PR is 400 miles.
The height of A above the sea level is 5 miles.
The length of the compressed portion is here represented as about 100 miles.

have become thickened, accompanied with internal corrugation and disturbance: and the thickening will die out gradually towards RS. This corrugated portion is represented in the diagram as about 100 miles across, but it would have been better had space permitted to have drawn it wider.

If the neutral zone between PQ and RS were now to maintain its horizontality, we should have the volume elevated above the mean line PR : that depressed below the mean line QS :: 2 : 3.[1] Such a condition of horizontality would require that $\Sigma(b) = 0$ and $\Sigma(\alpha) = 0$. It would also give for the condition of floating equilibrium,

$$\frac{\Sigma(a) - \Sigma(b)}{\Sigma(\beta) - \Sigma(\alpha)} = \frac{2}{3},$$

which would require that $\dfrac{\sigma - \rho}{\rho} = \frac{2}{3}$, or 0·666, and the subjacent fluid would need to be $\frac{5}{3}$ times as dense as the crust. This is altogether unlikely. We have on the other hand hitherto assumed,

[1] p. 135.

on grounds likely to make σ too large, that $\frac{\sigma - \rho}{\rho} = 0{\cdot}104$. The crust must then be so rigid that it cannot bend or break (and the points Q and S are not fixed points, but may retreat from one another so as to give any requisite smallness to the curvature of the bent tract between them) or else it must either bend or break, and hollows corresponding to $\Sigma(b)$ be formed in which the ocean would exceed the average depth. It is not likely, from the nature of the case, that any will be formed below, belonging to the series $\Sigma(\alpha)$. It will consequently take such a position as indicated in the diagram, in which the condition is intended to be approximately satisfied that

$$\frac{\Sigma(a) - \Sigma(b)}{\Sigma(\beta)} = 0{\cdot}104.$$

The volume A above the ocean level is supposed in the diagram to be about five miles high.

Here then we have exhibited the primary idea of the formation of a mountain ridge according to our theory.

Let us next proceed to enquire what effects would be produced upon our tract by denudation, and the transference of sediment.

It has been shown that

$$\Sigma(a) - \Sigma(b) = \frac{\sigma - \rho}{\sigma} kle + \frac{\mu}{\sigma}\{\delta l - \Sigma(d)\}.$$

Hence $\Sigma(a)$ is always greater than $\Sigma(b)$; so that even if sufficient material was denuded off $\Sigma(a)$ and carried into $\Sigma(b)$ to completely fill it up, $\Sigma(a)$ would not have been all carried away. This shows that, when once an elevated tract has been formed, it must be permanent. The presence of an ocean also, since the term in μ is necessarily positive, will render the excess of $\Sigma(a)$ over $\Sigma(b)$ greater than if there were no ocean.

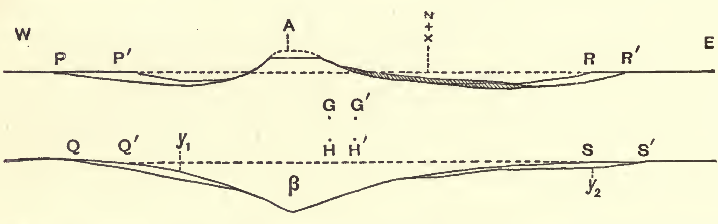
It does not however follow that, after $\Sigma(b)$ is filled up, $\Sigma(a)$ may not be denuded down below the ocean level, which it must be carefully remembered is distinct from, and above, the upper mean level. All that we assert is that an elevation

once formed must be permanent above the mean level, so far as denudation can affect it.

In the position which the tract has now assumed, there will be at first a certain amount of residual horizontal pressure at *PR* and *QS*, because the rigidity of the crust opposes a resistance to the horizontal thrust, which is only able to distort it to a certain degree. Since however, with sufficient time allowed, any substance not absolutely rigid will gradually yield to distortion under the action of pressure, we may assume that eventually the pressures at *PR* and *QS* will disappear, and the mass of the tract be under the action of the vertical forces only; and that these will be in equilibrium.

As we have drawn our diagram the centre of gravity of the mass bounded by *PQ* and *RS* will be at *G*. And since the resultant of the vertical forces acts through *G*, it follows that if the cross stresses at *PQ* and *RS* are just such as the crust will bear, and equal, *G* will be equidistant from *PQ* and *RS*.

We have now arrived at the conception of a ridge, of which our diagram represents a section, steeper upon one side, say the west, than upon the other, the east. It is bordered by an ocean upon either side. The ocean extends beyond the limits of the tract disturbed, which is defined by *P* and *R*, and is there of its average depth. But there are channels, corresponding to $\Sigma(b)$, running parallel to the ridge, where the



In this diagram the vertical scale is double the horizontal. The latter is somewhat reduced from the scale of the previous diagram.

The shaded part is the sediment.

The ocean level is omitted; and no attempt has been made to represent the changes of level, which, although of the greatest importance, would have been scarcely visible upon the figure.

ocean is abnormally deep; and of these the deeper is upon the side where the ridge is steeper. That this will be the case is obvious, on account of the position of the centre of gravity of the tract being half way between PQ and RS.

When atmospheric influences begin to act on the ridge, and denudation comes into play, we may expect that the principal and longer streams will be formed on the east side, which is less steep. The crest of the ridge will then be lowered, the basset edges of the contorted strata exposed, and the material removed will be deposited on the low grounds, and along the shore line, more on the east side than on the west.

We must remember however that our diagram represents only a section across an isolated ridge. But it will be proper to contemplate the existence of other elevated tracts, which, either by rivers flowing from them, or by ocean currents, or by shore drifts, or by iceberg deposits, may contribute to the deposition of sediment to the east of the ridge. For instance, in the case of a section taken from Valparaiso to Buenos Ayres, we should need to consider the sediment brought down by the Rio de la Plata, which would not be derived from any area crossed by the line of section.

In endeavouring to trace the mechanical results of such denudation and deposition, let us call x the material denuded off the ridge A, and z that brought from some distant region and deposited along with x. If we assume the centre of gravity of the tract to be originally at G, then the denudation of the material x off A, and its transference towards the east, will shift this centre towards the east. The deposition of the additional sediment z, brought from a distance and deposited along with or near x, will have a like additional effect. Let G' be the position to which this centre is shifted.

If the crust were flexible, this would produce deformation, and an alteration of curvature on either side of G'; but it would not tend to cause rotation. Since however it is only partially flexible, the deformation will proceed as far as the forces acting will carry it, and when they can bend it no further,

it will afterwards behave as if it were rigid. Suppose at this stage that the centre of gravity of the displaced fluid is brought to H. This point will evidently be to the east of G'. The upward pressure of the fluid at H will tend to produce rotation round G', raising the tract to the west, and depressing it towards the east. One of two effects may be expected to result. Either the crust will break off at RS and perhaps also at PQ, where before the change of pressure it was only just able to bear the downward stress, or else, as in the diagram, the limits of the depressed tract will be shifted eastward to $P'Q'$ and $R'S'$. In either case, an additional volume of the crust will be depressed into the fluid towards the east, and the effect will be, that the centre of gravity of the displaced fluid will be also shifted towards the east; in which direction it will move, until equilibrium is restored by its arriving beneath G'.

The centre of gravity of the tract will thus continually travel eastwards, as more and more sediment is deposited in that direction. And since the centre of gravity is the point, about which the tract will tend to rotate under the action of the shifted and of the additional weight above, and of the fluid pressure beneath, it follows that, if the ends of the tract at PQ and RS are not tilted upwards and downwards, as would happen if the tract were rigid, there will be a kind of wave of elevation travelling from west to east, whose crest will carry with it the centre of gravity of the tract. We see then *a priori* that the region which receives the sediment from A will sink, but at the same time A from which it came will rise, and if there be additional sediment brought from distant regions, these effects, not only the sinking of the sedimented parts, but the rise of A, will be intensified. We might even expect, that some of the sediment earliest deposited nearest the west, might be raised above the ocean by this rotational action alone.

We will now endeavour to obtain a somewhat closer insight into the character of the movements, in doing which we shall for simplicity neglect the weight of the ocean.

If a mass floats in a fluid, as we have supposed our tract virtually to do, the densities being respectively ρ and σ, we

know that, whether the mass be rigid or not,

$$\rho \times \textit{whole volume} = \sigma \times \textit{volume immersed.}$$

And, since the centres of gravity of these volumes must be in the same vertical, if we multiply each side of the above by the distance of its corresponding centre from any assumed vertical we have, considering the moments about that vertical,

$$\rho \times \textit{moment of whole volume} = \sigma \times \textit{moment of volume immersed.}$$

Let A be the volume which before denudation stood above the ocean level, and, as already supposed, let a mass x be denuded off it, and carried down towards the east, and let another mass z be also brought from a distance and deposited along with x. This, as has been explained, will tend to depress the tract on the eastern side, and to raise it on the western, and our object is to determine as far as possible the situations and amounts of the depression and elevation.

Let y_2 be the additional volume of crust depressed beneath the lower mean level on the eastern side, and y_1 the volume by which the portion previously depressed is now diminished on the western side. The whole volume beneath the lower mean level will now be $\beta - y_1 + y_2$.

Let us call the horizontal distances, from the assumed vertical, of the centres of gravity of the volumes A, $x+z$, y_1, y_2, respectively $\bar{A}$, $\bar{x}$, $\bar{y}_1$, $\bar{y}_2$.

Let B be the volume of the tract which is exclusive of A, and β the volume at first below the lower mean level; $\bar{B}$ and $\bar{\beta}$ the distances of their centres of gravity from the assumed vertical line. Then the relation just proved gives us, before the transference of sediment,

$$\rho\,(A\,.\,\bar{A} + B\,.\,\bar{B}) = \sigma\beta\,.\,\bar{\beta}.$$

After the transference of x and deposition of z, A becomes $A - x$, and we may suppose the distance of the centre of gravity of the diminished A to be unaltered by this change. B and $\bar{B}$ remain as before. β is diminished by y_1 and increased by y_2. Hence the relation of the moments will now be

$$\rho\,\{(A - x)\,\bar{A} + (x + z)\,\bar{x} + B\,.\,\bar{B}\} = \sigma\,(\beta\,.\,\bar{\beta} - y_1\cdot\bar{y}_1 + y_2\cdot\bar{y}_2),$$

whence, subtracting,

$$\rho \{(x+z)\,\bar{x} - x\bar{A}\} = \sigma\,(y_2\bar{y}_2 - y_1\bar{y}_1).$$

If then we assume the vertical to pass through the centre of gravity of the volumes y_1 and y_2, the second side will vanish, and we have

$$(x+z)\,\bar{x} = x\bar{A}.$$

This shows that the centre of gravity of the mass x before its removal, and of $x+z$ after their deposition in their new position, is in the same vertical with the centre of gravity of y_1 and y_2. Hence we can find the position of the centre of gravity of these latter volumes but not the position of the volumes themselves. It shows that the centre of gravity of y_1 and y_2 travels eastward at exactly the same rate as that of x before denudation and $x+z$ after deposition.

We may next compare the rate at which the centre of gravity of the whole tract travels, relatively to the vertical through the centre of gravity of y_1, y_2. For if M be the mass of the whole tract before the transference and deposition of sediment, and consequently $M+z$ afterwards, $\bar{M}$, $\bar{M}'$ the distances of their centres of gravity from an assumed vertical, then we have at first

$$M\,.\,\bar{M} = x\bar{A} + B\,.\,\bar{B};$$

and afterwards

$$(M+z)\,\bar{M}' = (x+z)\,\bar{x} + B\,.\,\bar{B}.$$

But if we measure from the vertical which passes through the centre of gravity of y_1, y_2, we know that

$$x\bar{A} = (x+z)\,\bar{x};$$

$$\therefore\ (M+z)\,\bar{M}' - M\bar{M} = 0.$$

If $z=0$, then $\bar{M}' = \bar{M}$.

So that by the transference of x alone, the centre of gravity of the whole mass remains at a constant distance from the vertical through that of y_1, y_2, not depending upon the amount of x.

If z is considerable compared to M, we see that $\overline{M}'$ is less than $\overline{M}$, and the centre of gravity after transference is nearer to the vertical through that of y_1, y_2 than it was before. But since M must be always much larger than z, it will not approach very near to it. For instance, even if $z = \frac{1}{2}M$, $\overline{M}'$ would be $\frac{2}{3}\overline{M}$.

This result shows that the rotation round G', which will depress the centre of gravity of $x + z$, would not be likely to bring up any of this sediment above the sea level, unless $x + z$ was spread out very thin, so that its western boundary considerably overlapped the point G'.

But although an elevatory effect upon the newly deposited sediment can hardly be expected to result from the *rotation* of the tract round G', nevertheless there is a further consideration which leads us to expect that this consequence will follow upon the transference of sediment from a distance, for the following reason.

It is evident that we must regard the tract under consideration, whose volume was $kl(1+e)$ before it received the new accession z, as now become $kl(1+e) + z$. Hence, if $\Sigma(a)$ and $\Sigma(b)$ become now $\Sigma(a')$ and $\Sigma(b')$ we have, neglecting the effect of the ocean[1],

$$\Sigma(a') - \Sigma(b') = \frac{\sigma - \rho}{\sigma}(kle + z);$$

$$\therefore\ \Sigma(a') - \Sigma(a) - \{\Sigma(b') - \Sigma(b)\} = \frac{\sigma - \rho}{\sigma} z.$$

We see then that the absolute volume above the upper mean level, and therefore probably also above the ocean, is in consequence of the accession of z increased by $\frac{\sigma - \rho}{\sigma} z$, or by about $\frac{1}{10}$th of the new accession of material to the tract.

In a similar way it appears that the volume below the lower mean, or $y_2 - y_1$, must be increased by $\frac{\rho}{\sigma} z$, or by $\frac{9}{10}$ths of the new accession. Although therefore there will be an addition to the absolute volume above the upper mean level, and

[1] See the last equation on p. 116.

possibly above the ocean, there will be at the same time an addition of about nine times as much to the volume below the lower mean. Or nine-tenths of the sediment brought down will disappear from view. This explains how it is, that much of the enormous quantity of sediment brought down by a great river appears to be as it were swallowed up in a bottomless abyss. It will be recollected, that this was one of the facts primarily adduced to show *a priori* the improbability of an entirely solid earth[1].

We appear to have now learnt all we are able about the character of the movements produced by the transference of the sediment. We cannot find the actual positions of the volumes y_1 and y_2, because these will depend upon the degree of rigidity of the crust. And even if we knew what this is, the determination would probably be impracticable. However, we can see that the less readily the crust replies by bending or breaking to the altered strain upon it, the less the curvature will be altered, and therefore the further will be the centres of y_1 and y_2 from the axis of rotation of the tract.

A consequence of the rotation round G' will be to give the tract a certain degree of inclination, or "hang" towards the east, so that the resultant pressure of the fluid will have a certain amount of eastward direction; and in assuming the new position of equilibrium there will be a general shifting of the whole tract eastwards. The result (and it is a very important one) will be to subject the crust in the region of y_1 to tension, which will possibly open fissures downwards on the western side of the ridge.

The smaller the distance apart of y_1, y_2, the greater this "hang" will be for given values of them.

It is evident that the existence of y_1, the uptilted volume, depends entirely upon the crust possessing a certain degree of rigidity. If there were no rigidity there would be no tilting. A certain additional rigidity will be imparted to the tract as a whole, by the existence of a considerable ridge, which, with

[1] p. 81.

its accompanying depression, will act as a brace to prevent bending, strengthening the tract in the neighbourhood of the *quasi* fulcrum at the centre of gravity.

The removal of material off *A* by denudation, independently of its subsequent deposition towards the east, will have the effect of shifting *G* to the east, and causing the ridge to be lifted by the pressure of the fluid beneath. The mere degradation of a mountain chain, without taking into account the tilting caused by the weight of sediment in the bordering hollows, would consequently be accompanied by a corresponding rising of that part of the mass.

CHAPTER XI.

THE REVELATIONS OF THE PLUMB-LINE.

Mountains the "backbones" of continents—They have "roots"—These are revealed by the plumb-line—Pratt's calculation of the attraction of the Himalayas—The actual attraction less than that calculated—His attempted explanation of the discrepancy—Sir G. B. Airy's explanation more satisfactory—Pratt's reply shown to be inconclusive—The result confirms the reasoning of the preceding chapter—The plumb-line reveals a greater density beneath oceans.

WE have now arrived at a point at which we may begin to test our theories by comparing the results of them with observed phenomena.

In noticing the section on p. 132, which is roughly drawn to scale, we are at once struck with the importance assumed by a mountain range with reference to a continent, when the depression below the lower mean level is taken account of, as well as the elevation which appears above the level of the sea. The latter has been usually regarded as the sufficient expression of the magnitude of the range, and the importance of the highest mountains has accordingly been minimized, and they have been called mere wrinkles upon the surface of the globe. Wrinkles no doubt they are; but they must be regarded as thicker, than they appear above ground to be, in the ratio of perhaps 5 to 2, and they may be fairly called the backbones of continents.

According to the consequences which we have traced, as resulting from their denudation, we are also led to look upon a mountain range as truly the parent of a continent, and to

consider the latter as gradually evolved out of the former: so that, even if there is no present range of sufficient altitude to be dignified as a mountain chain, such must have at some former time existed.

We also see why a range of mountains as a rule skirts an ocean shore, or at least did so when it was first formed. And we also see why it usually presents its steeper face towards the ocean.

It is not a little remarkable that popular expressions, even when grounded on no scientific basis, have often a foundation in truth which justifies them. Such an expression is that which speaks of the "roots of the mountain." This is amply justified by our theory; for every mountain must possess a corresponding protuberance projecting downwards into the fluid substratum.

These roots of the mountains, though they cannot be seen, can in a most remarkable manner be felt by the plumb-line; and this certainly affords a strong support to the truth of our theory. "The attraction of the Himalaya Mountains, and of the elevated regions lying beyond them, has a sensible influence upon the plumb-line in North India. This circumstance has been brought to light during the progress of the great trigonomatrical survey of that country. It has been found by triangulation that the difference of latitude between the two extreme stations of the northern division of the arc," that is, between Kalianpur and Kaliana, "is $5^{\circ}\ 23'\ 42''\cdot294$, whereas astronomical observations shew a difference of $5^{\circ}\ 23'\ 37''\cdot058$, which is $5''\cdot236$[1] less than the former[2]." The latitude of Kalianpur is $24^{\circ}\ 7'\ 11''$ and that of Kaliana $29^{\circ}\ 30'\ 48''$.[3]

It having thus appeared that the plumb-line was attracted at the station near the mountains in a direction towards the

[1] "This is the difference as stated by Colonel Everest in his work on the measurement of the meridianal arc of India published in 1847. See p. clxxviii."

[2] "On the Attraction of the Himalaya Mountains, and of the elevated regions beyond them upon the Plumb-line in India." By the Venerable John Henry Pratt, M.A., Archdeacon of Calcutta. *Phil. Trans. of Royal Soc.* Vol. 145, p. 53.

[3] *Ibid.* p. 56.

mountains, thereupon Archdeacon Pratt set himself what appeared the Herculean task of actually calculating what the effect of the attraction of that great mountainous region ought to be, and in a second paper upon the same subject he says, "My calculation has been before the public three years; and, though some small numerical errors have been detected, they are not of sufficient importance to affect the result; and the data I have every reason for believing to be correctly taken, as the Surveyor-General—who first called my attention to the subject in 1852, as an unsolved difficulty in the operations of the great Trigonometrical Survey of India—has been requested to forward to me any corrections which may appear to him to be advisable, and none have been sent[1]." The result at which he arrived was, that the attraction of the mountainous region ought to have made, between the true and astronomically observed differences of latitude of the extreme stations, a discrepancy of $15''\cdot885$ instead of $5''\cdot236$ as it did. That is to say, the effect of the attraction of the mountains was in fact considerably less than it ought to have been, according to what their mass above the sea should produce, taking 2·75 from Maskelyne's determination for Schehallion for the density of the attracting mass. Thus the mountains' attraction not only accounted for the discrepancy discovered by the Survey, but accounted for too much. They ought to have attracted the plumb-line more than they did.

The Archdeacon then applied himself to explain this unexpected anomaly, and the conclusion he came to in his first paper was, that the assumed ellipticity $\frac{1}{300\cdot8}$ was too large for the Indian arc, the curvature there having been, as he thought, probably increased by the upheaving of the mountains. This would have the effect of making the difference calculated upon the triangulation larger, so as to compensate for the effect of the mountains[2].

[1] Second Paper by Archd. Pratt. *Phil. Trans. of Royal Soc.*, Vol. 149, p. 746.

[2] If λ be the amplitude of the arc (*i.e.* the difference between the latitudes of its extreme stations), l its length, μ the latitude of its middle point, and ϵ the

However the very next paper in the *Phil. Transactions*[1] disposes of the difficulty in a very satisfactory manner. It was communicated by the late Astronomer Royal about a year after Archdeacon Pratt's great paper appeared. This paper is short and simple, and with the author's permission the following passages are extracted from it.

"Although the surface of the earth consists everywhere of a hard crust, with only enough of water lying upon it to give us everywhere a *couche de niveau,* and to enable us to estimate the heights of the mountains in some places, and the depths of the basins in others; yet the smallness of those elevations and depths, the correctness with which the hard part of the earth

ellipticity, the usual formula is

$$\frac{\text{length of arc } (l)}{\text{equatorial radius } (a)} = \lambda - \tfrac{1}{2}\epsilon\,(\lambda + 3\sin\lambda\cos 2\mu).$$

λ is also affected by the mountain attraction, which we may call H.

Consequently

$$\lambda = f\,(a,\ l,\ \mu,\ H,\ \epsilon).$$

Consequently if the observed value of λ be not such as accords with the usually assumed value of ϵ when H is determined, some different value of ϵ may be found which will bring λ into accordance with its observed value. And this is the mode in which Archd. Pratt proposed to explain the difficulty.

Eliminating a from the above equation by means of another measured arc, the latitude of whose middle point is M, he obtains the following approximate formula:

$$d\epsilon = \frac{d\lambda}{\lambda}\,\frac{2}{3\,(\cos 2\mu - \cos 2M)}\,.$$

In order to give this as small a value as practicable he takes the Russian arc for the other, for which $M = 70^0$, and after applying the formula to the case in hand, finds

$$d\epsilon = \frac{1}{39585} = \frac{\epsilon}{132}\,,$$

if we put $\frac{1}{300}$ for ϵ.

"Hence for an error of $5''\cdot236$ in defect in the amplitude, the effect on the ellipticity will be to diminish it by $\frac{5\cdot236}{132}\,\epsilon = \frac{\epsilon}{25}$ nearly, or by nearly $\frac{1}{25}$ part of its whole value under the most favourable circumstances."

[1] "On the computation of the Effect of the Attraction of Mountain-masses as disturbing the Apparent Astronomical Latitude of Stations in Geodetic Surveys." By G. B. Airy, Esq., Astronomer Royal. Feb. 15, 1855. *Phil. Trans. Royal Soc.*, Vol. 145, p. 101.

has assumed the spheroidal form, and the absence of any particular preponderance either of land or of water at the equator as compared with the poles, have induced most physicists to suppose, either that the interior of the earth is now fluid, or that it was fluid when the mountains took their present forms. This fluidity may be very imperfect, it may be mere viscidity; it may even be little more than that degree of yielding which (as is well known to miners) shows itself by changes in the floors of subterraneous chambers at a great depth where their width exceeds 20 or 30 feet, and this yielding may be sufficient for my present explanation."

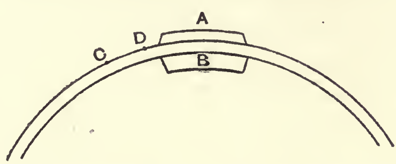
He then proves that a table-land, say of 100 miles broad and two miles high, could not rest upon the surface of a crust ten miles thick.

"Now I say that this state of things is impossible, the weight of the table-land would break the crust through its whole depth from the top of the table-land to the surface of the lava" (which is merely used as a generic term for a heavier subjacent non-solid material), "and either the whole or only the middle part would sink into the lava." To prevent this happening, with the assumed dimensions, it would be necessary that the cohesion of the rock should be sufficient to support a suspended column of the same twenty miles long. "I need not say that there is no such thing in nature. If instead of supposing the crust ten miles thick we had supposed it 100 miles thick, the necessary value of the cohesion would have been reduced to $\frac{1}{5}$th of a mile nearly. This value would have been as fatal to the supposition as the other[1]."

"Yet the table-land does exist in its elevation, and therefore it *is* supported from below. What can the nature of its support be? I conceive there can be no other support than that arising from the downward projection of a portion of the earth's light crust into the dense lava; the horizontal extent of that projection corresponding rudely with the horizontal extent of the table-land, and the depth thus gained is roughly equal to the increase of weight above from the prominence of the table-land[2]."

[1] *Ibid.* p. 102. [2] *Ibid.* p. 103.

It will be at once seen that this reasoning produces an arrangement of the crust upon the subjacent fluid exactly analogous to that which has been arrived at in the preceding chapter. Of course we from our point of view cannot contemplate the independent formation of a table-land apart from the downward projection, so that it should afterwards break and form one. But it does not seem that it concerned Sir G. B. Airy to show that the one cannot be formed without the other. The object he had in view was to explain how this arrangement of matter would produce a smaller deviation of the plumb-line. He says, "It will be remarked that the disturbance depends on two actions; the positive attraction produced by the elevated table-land, and the diminution of attraction produced by the substitution of a certain volume of light crust (in the lower projection) for heavy lava. The diminution of attractive matter below, produced by the substitution of light crust for heavy lava, will be sensibly equal to the increase of attractive matter above. The difference of the negative attraction of one and the positive attraction of the other, as estimated in the direction of a line perpendicular to that joining the centres of attraction of the two masses (or as estimated in a horizontal line), will be proportional to the difference of the inverse cubes of the distances of the attracted point from the two masses."



"Suppose then that the point C is at a great distance, where nevertheless the positive attraction of the mass A, considered alone, would have produced a very sensible effect on the astronomical latitude, as ten seconds. The effect of the negative attraction of B will be $10'' \times \frac{CA^3}{CB^3}$; and the whole effect will be $10'' \times \frac{CB^3 - CA^3}{CB^3}$, which probably will be quite insensible.

"But suppose that the point D is at a much smaller

distance, where the positive attraction of the mass A would have produced the effect n''. The whole effect, by the same formula, will be $n'' \times \frac{DB^3 - DA^3}{DB^3}$, or $n'' \times \left(1 - \frac{DA^3}{DB^3}\right)$; and as in this case the fraction $\frac{DA}{DB}$ is not very nearly equal to 1, there may be a considerable residual disturbing attraction. But even here, and however near to the mountains the station D may be, the real disturbing attraction will be less than that found by computing the attraction of the table-land alone."

The extreme felicity of the above explanation of a remarkable phenomenon and the lucidity with which it is stated must be sufficient apology for the length of the extract. One remark more must be added because it is very pertinent to the subject and reasoning of the preceding chapter. "It is supposed that the crust is floating in a state of equilibrium. But in our entire ignorance of the *modus operandi* of the forces which have raised submarine strata to the tops of high mountains, we cannot insist on this as absolutely true. We know (from the reasoning above) that it will be so to the limit of *breakage* of the table-lands, but within those limits there may be some range of conditions either way[1]." We believe however that since this passage was written geology has made some advance towards explaining the *modus operandi* of the elevatory forces; but the proviso respecting the limits of breakage of the rocks is of great importance and must not be forgotten.

Archdeacon Pratt replied to the Astronomer Royal's paper three years later[2]. He brought against the explanation proposed three objections, not one of which appears in the present state of our knowledge to be of weight[3]. (1) It supposes the thickness of the earth's solid crust to be considerably smaller than Mr Hopkins concluded it to be. This objection has been disposed of[4]. (2) It assumes that the crust is lighter than the fluid on which it rests, whereas in becoming solid we should expect it to

[1] *Loc. cit.* p. 104.

[2] "On the Deflection of the Plumb-line in India, &c." *Phil. Trans. Royal Soc.*, Vol. 149, p. 745.

[3] *Ibid.* p. 747. [4] p. 22.

contract and become more dense. This argument is answered by Mr Whitley's experiments[1], and is contrary to Waltershausen's most reasonable theory[2] that the successive couches increase in density according to their chemical composition. (3) If every protuberance outside a thin crust must be accompanied by a protuberance inside down into the fluid mass, then wherever there is a hollow as in deep seas in the outward surface, there must be one also in the inner surface of the crust corresponding to it; thus leading to a law of varying thickness which no process of cooling could have produced. But we appeal to a mode of action, namely compression, which is quite capable, in the case of a crust not perfectly soft and compressible, of producing such surface hollows[3] and corresponding downward protuberances, as we have explained in the preceding chapter. In spite however of these three objections, which the Archdeacon believed to be fatal to the form in which the Astronomer Royal offered his explanation of a deficiency of matter below the mountains, the Archdeacon adopts in the remainder of this paper another form of the same theory, apparently preferring it altogether to his former hypothesis of an increased ellipticity for the Indian arc.

We have thus established our assertion that the roots of the mountains can be felt by means of the plumb-line.

The converse proposition ought to be true, that where the land is not elevated above the sea, there the heavier substratum ought to be within a less distance of the surface, and the consequence ought to be that attraction at the surface should be greater. This also is known experimentally to be the case. The subject is discussed by Archdeacon Pratt in the fourth edition of his *Figure of the Earth.* He establishes the fact, but raises upon it theoretical conclusions not warranted by geology. There can however be no doubt that he has demonstrated the existence of increased density beneath the oceans. He says "in fact the density of the crust beneath the mountains must be less than that below the plains and still less than that below the

[1] p. 23. [2] p. 28. [3] p. 133.

ocean bed[1]. "That part of the theory" (of unequal radial contraction, which he proposes but we disbelieve[2]) "which shows that the wide ocean has been collected on parts of the earth's surface where hollows have been made by the contraction and therefore increased density of the crust below, is well illustrated by the existence of a whole hemisphere of water, of which New Zealand is the pole, in stable equilibrium. Were the crust beneath only of the same density as that beneath the surrounding continents, the water would be drawn off by attraction and not allowed to stand in the undisturbed position it now occupies." The greater density must then clearly exist; but it will appear in the following chapter[3], that the phenomena are not sufficiently explained by supposing the lighter crust thinner, and the heavier substratum nearer the surface in those regions.

[1] "Figure of the Earth." 4th Ed. Art. 192, pp. 201, 202.
[2] p. 79.
[3] p. 164.

CHAPTER XII.

THE REVELATIONS OF THE THERMOMETER.

Recapitulation of results of previous chapter—Roots of mountains should be revealed by phenomena of underground temperature—Conditions upon which the mean rate of increase of temperature will depend—Rate greater in plains and less in mountains—Dr Stapff—His observations on temperature of rocks in St Gothard tunnel—His explanation of the smallness of the rate—Why not satisfactory—Effect of convexity of mountain upon the rate—The rate may be considered uniform above—The conclusion from the premises is that the mountain has roots—The rate may be regarded as nearly uniform throughout—General method of determining the thickness of the crust at the sea-level, and the melting temperature, from the rates beneath the mountain and the usual rate—Application to St Gothard—Thickness of crust at sea-level and melting temperature deduced numerically from the data—The like for Mont Cenis—Results confirmatory of previous conclusions—Considerations about the oceanic areas—How the water is retained there—The thickness of the sub-oceanic crust and its density.

We have seen in the preceding chapter that certain results of geodesy agree well with the conclusion arrived at in the tenth chapter, that mountains have "roots," that is that there is, for all regions elevated above the mean level of the crust, a corresponding downward protrusion of the lighter material of the crust into the heavier substratum. This is in substance the same thing which Herschel expressed by saying that, "the force by which continents are sustained is one of tumefaction[1]." According to our theory these roots will consist of the lighter material of the crust, in an unmelted state, projecting into a heavier molten fluid. The amount of this projection may be roughly determined by the relative densities of the crust and fluid, in the same way as if the crust floated without constraint in equilibrium. For although, as we have argued in the tenth

[1] See p. 76.

chapter, the partial rigidity of the crust will render this assumption in strictness incorrect, yet the deviations from accuracy consequent upon it cannot amount to anything considerable when compared to the whole thickness; and this will be especially true in the neighbourhood of the chains, near which the centre of gravity of a disturbed tract will lie; and which will therefore be least depressed or elevated by any slight tilting action.

If this be a true statement of the case, then the existence of these roots ought to be revealed in another and entirely distinct manner, namely by the phenomena of underground temperature. Where the roots of the mountains are in contact with the molten fluid, they must be slowly and gradually becoming fused. At other localities, where there is no such protuberance, the bottom of the mean crust must either remain sensibly at a permanent temperature, which will be that of solidification (the same as of fusion), or else it will be slowly and gradually freezing, and growing thicker. At all localities we shall therefore have, maintained at the bottom of the solid crust, sensibly the melting temperature of the materials of which the crust is there composed, modified perhaps by the difference of pressure at their different depths, which we need not for the present purpose consider.

Now it is evident that, if the two surfaces of a plate of any substance are maintained at two different constant temperatures, the mean rate of increase of temperature within the plate will depend solely upon its thickness irrespectively of its conductivity. The mean rate therefore of increase of temperature within the crust, at any locality where we may suppose these conditions approximately fulfilled, will depend upon the difference of the temperatures of its upper and under surfaces at that locality, and on its thickness, and not upon the conductivity of the material, nor upon the absolute temperatures. For although contortions in the bedding, or difference of composition, may affect the rate as between one level and another at that locality, still they will not affect the mean rate. Nor yet need such geological accidents be considered in comparing the mean rate at one place with that at another. The

result is that the mean underground rates at all places at the same height above the sea, and where the mean annual surface temperature is the same, ought to be equal, irrespectively of the nature of the rocks, provided only that the temperature at the bottom of the crust is the same.

But if on the other hand the heights of two localities be different, the mean rates there ought to differ, not in inverse proportion to the heights above the sea, but approximately in inverse proportion to the total thicknesses of the crust, including the roots down to the molten substratum. Is there any reason to believe that this is the case? If we find it to be so, we shall have a presumption, in favour of our theory, not only that there is a downward protuberance of lighter material, as already shown to be corroborated by geodesy, but that this protuberance consists of unmelted crust projecting into molten fluid.

Of course the above reasoning requires the state of temperature to be sensibly permanent, and that a sufficient time should have elapsed, since the mountains were elevated, for the heat brought up from below along with the uplifted matter to have become so far dissipated, that the flow of heat through the crust has become steady.

Mr Mallet, in his paper on Volcanic energy[1], refers to a dissertation, which he considers "the most complete and valuable collection and discussion of all the observations on record up to June, 1836," by A. Vrolik, who, he states, "believes it proved, that in general the rate of increment is greater in plains and valleys than in mountains."

The great engineering achievements of the present time have afforded exceptional facilities for testing this question; and the above statement has been found to be strictly true. An extensive series of observations was carried out more especially in connection with the construction of the tunnel through the Alps at Mont St Gothard, by Dr F. M. Stapff, geological engineer

[1] *Phil. Trans. Royal Soc.*, Vol. 163, p. 158.

The title of the dissertation is "Disputatio physica Inauguralis de Calore telluris infra superficiem augescente." 4to., 101 pages. Müller, Amsterdam, 1836.

to the company; and very interesting results were obtained, which are recorded in his published papers[1].

Dr Stapff points out that the rate of increase of temperature beneath the mountain differs from that beneath the plane surface, and gives it as his opinion that the influence which the mass of the mountain superimposed, and free on each side, exercises on the temperature of the rocks, is very different from that which an envelope would exercise, formed of the crust of the earth of a thickness equal to the height of this same mass[2]. This conclusion is ostensibly corroborated by the fact, that the augmentation of temperature is observed to be more rapid beneath the hollows and flat spaces of the open surface above the tunnel than beneath the summits.

It may however be shown that the contour of the mountain cannot account for a general rate of increase of temperature beneath it so much slower than the usual one. The mean rate at Mt St Gothard for the whole tunnel is found by Dr Stapff to be $0^{\circ}{\cdot}0206$ C. per metre of descent, which is about $\frac{1}{88}$ degree F. per foot[3]. This is little more than half the usual rate of increase. The rate was found to exceed this mean on the north more often than on the south side of the mountain.

To estimate roughly the effect which the contour of the mountain would produce in lowering the rate of increase, we may argue thus. As we ascend in the atmosphere the temperature decreases. The temperature of the summit of the mountain is found to be zero centigrade, or the freezing-point. Calculate then the altitudes at which the observed rate at

[1] "Studien über die Wärmevertheilung im Gotthard." Bern, 1877. "Étude de l'Influence de la Chaleur de l'Intérieur de la Terre sur la possibilité de construction des Tunnels dans les hautes Montagnes. Première partie, 1879. Do. deuxième partie, 1880. Revue universelle des Mines, etc. Annuaire de l'Association des Ingénieurs." Paris, 9, Rue des Saints-Pères. Londres, 5, Bouverie Street. Liége, 24, Rue d'Archis. "Repartition de la température dans le grand tunnel du St Gothard. Annexe XIV au volume VIII des rapports trimestriels du Conseil fédéral sur la marche des travaux du chemin de fer du St Gothard (rapport N°. 30)." 1880.

[2] "Étude de l'Influence de la Chaleur de l'Intérieur de la Terre," &c., p. 2.

[3] The factor which reduces the former to the latter system of units is 0·548.

equal horizontal distances along the tunnel would bring us to the freezing-point within the rock supposing it extended to a sufficient height, and draw a curve through these points, which will touch the summit of the mountain. This curve will define what may be called the effective profile of the mountain, that is to say, the form of outer surface, which would give the same temperatures within the mountain as actually exist, were the temperature of the surface uniform. It will be seen that the contour so obtained is very considerably less convex than the actual contour. This shows that the real effect of the convexity of the surface upon the internal temperatures cannot be so great as at first sight it might appear likely to be.

The isogeotherms within the mountain will evidently consist of a family of surfaces corresponding to the external effective profile, but decreasing in convexity with increasing depth. Eventually a plane horizontal isogeotherm will be reached. But it does not follow that the isogeotherms will continue to be planes beneath it.

Let S be the area of one of these surfaces, through which the same heat passes in a given interval of time as that which crosses the area A of the plane isogeotherm in the same interval. Let n be measured along the normal drawn to a point in the surface S, v being the temperature at that point, and κ the conductivity. Then the flow of heat across the elementary area dS will be

$$\kappa \frac{dv}{dn} dS.$$

Properly speaking $\frac{dv}{dn}$ will vary from point to point of the isogeotherm. But if we consider it to be the average rate along the normal at every point of a given isogeotherm, then the flow across the whole of that surface will be

$$\kappa \frac{dv}{dn} S.$$

But the same flow of heat crosses the plane area A, where let us call r' the average rate of increase of temperature in descending; wherefore the whole flow there will be $\kappa r' A$.

Hence we must have

$$\frac{dv}{dn} S = r'A,$$

$$\text{and} \therefore \frac{dv}{dn} = \frac{A}{S} r'.$$

This gives the proportion according to which the average rate along any normal to an isogeotherm is diminished in consequence of the convexity of its surface. This rate will clearly become more and more nearly equal to that through the plane isogeotherm, as that is approached.

Now the whole length of the St Gothard tunnel is 14920 metres, and the greatest height of the mountain above it is 1127 metres. It is therefore obvious that the ratio $\frac{A}{S}$ must be nearly one of equality. Indeed, if we suppose the contour of the surface of the mountain to be a segment of a circular cylinder, we shall find that the value of $\frac{A}{S}$ for the outer surface, where that ratio will differ most from equality, will be 0·996. And consequently, if we suppose that the temperature of this surface is everywhere 0° C. (an unfavourable supposition), then

$$\frac{dv}{dn} = 0{\cdot}996 r'.$$

This shows that the convexity of the mountain can only slightly affect the rate along the vertical anywhere, and in the central part, where $\frac{dv}{dn}$ becomes $\frac{dv}{dx}$, scarcely at all. We are therefore at liberty to regard the rate in the central part of the mountain as very nearly constant and equal to r'.

Now the temperature at the upper surface being 0° C., if h be the height of the mountain above the tunnel, and t the temperature of the rock in the centre of the tunnel, the rate

$$r = \frac{t}{h}.$$

But at the depth where the plane isogeotherm is encountered the temperature beneath the mountain must equal that of the

neighbouring crust, because the isogeotherm is plane. It is obvious therefore from geometry, the rates beneath the mountain and at any place in the neighbourhood being both of them uniform yet not the same, that this plane isogeotherm cannot be elsewhere than at the bottom of the crust, unless the isogeotherms beneath the mountain become concave upwards after the plane isogeotherm is passed. Suppose then the former to be the case, and that k is the mean average thickness of the crust, and k' its thickness beneath the mountain; at which respective depths (if the isogeotherms beneath the mountain do not become concave) the temperatures become equal. Then we must have,

$$\frac{k}{k'} = \frac{r'}{r}.$$

But observation shows that r' is about $\frac{1}{2}r$,

$$\therefore k' = 2k;$$

or the height of the mountain above the mean level of the crust would in such case be equal to the thickness of the crust. This would make the crust far thinner than it can be admitted to be; and therefore shows that the isogeotherms below the plane one must have a convexity downwards, answering to their convexity upwards in the upper portion. There must therefore be a protuberance of solid crust below corresponding to the elevation of the mass above. We see then that we have here an additional proof of the existence of roots to the mountains derived from thermal phenomena.

The downward protuberance will be much greater than the upward elevation, and therefore we perhaps ought not to assume that the extreme value of the ratio of $A : S$ will be so nearly one of equality below the plane isogeotherm as above it. But it is probable that the downward protuberance, although it may be ten times as deep as the other is high, will be very much wider, because the St Gothard mountain is only a remnant, carved by subaerial denudation out of a much more extended elevated tract, with which the root was originally conterminous: and whatever denudation, or what may be analogous to it, the roots of the mountain range may have undergone, it is not likely

that their contour should be reduced to the semblance of inverted mountains and alternating valleys, like those which characterise the upper surface of an Alpine region. The ratio of $A : S$ may therefore be still regarded as nearly one of equality even in the lower portions.

If we are at liberty to regard the rate beneath the mountain as approximately uniform, it is possible to estimate from the data the thickness of the crust at a place on the sea-level, and likewise the melting temperature at the bottom of it, if we assume a ratio for the densities of the crust and fluid substratum. For generally let

c be the depth of a point in the crust from the surface,

a the temperature at that depth,

b the temperature at the surface,

r the rate of increase of temperature in descending.

Then we have the general relation,

$$\frac{a-b}{c} = r \qquad (1).$$

If v be the rock-temperature in the tunnel, b' that at the top of the mountain, m the height of the mountain above the tunnel, r' the rate, we have in like manner

$$\frac{v-b'}{m} = r' \qquad (2),$$

whence r' may be obtained from the observed data.

Now suppose equation (1) to refer to some place A, anywhere near the sea-level, where the total thickness of the crust is c, and the rate the usual one r.

Then $$a = cr + b.$$

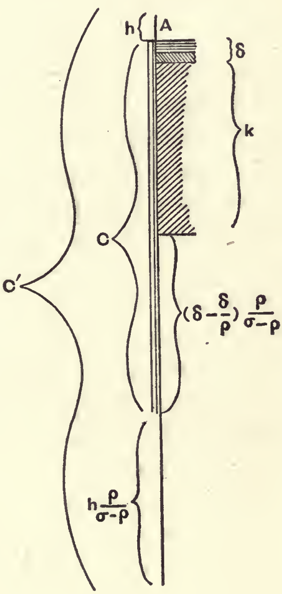
Similarly, if c' be the thickness of the crust at the mountain, the temperatures at the bottom of the crust being the same at both places because it is the melting temperature,

$$a = c'r' + b'.$$

Consequently

$$cr + b = c'r' + b' \dots\dots\dots\dots\dots\dots\dots\dots(3).$$

In this equation r, r', b, b' are known, and therefore, if we can obtain another relation between c and c', we shall be able to find the thicknesses of the crust at the place on the sea-level and at the mountain. If we are at liberty to apply the conditions of hydrostatic equilibrium, such a relation is easily obtained, as shown on the left-hand side of the figure, from which it appears that,



$$c' = h + c + h\frac{\rho}{\sigma - \rho},$$

$$= c + h\frac{\sigma}{\sigma - \rho}.$$

But from (3) we have

$$c' = \frac{r}{r'}c + \frac{b - b'}{r'},$$

whence

$$c = \frac{r'}{r - r'} \frac{\sigma}{\sigma - \rho} h - \frac{b - b'}{r - r'}, \ldots\ldots\ldots\ldots\ldots\ldots\ldots (4).$$

If we assume a value for the ratio $\frac{\rho}{\sigma}$, this expression gives the thickness of the crust at a place at the sea-level, where the surface temperature is b, and the rate r. The place ought not to be in the mountainous region, in order that the rate may not be affected by the additional thickness of crust beneath such a region.

Let us now apply equation (4) to find the thickness of the earth's crust at a place at the sea-level, from the data obtained from the St Gothard tunnel. In Table VIII. of Dr Stapff's paper[1], the rate at the exact middle of the tunnel is not given, but from the plate which illustrates his "Repartition de la Temperature" it can be found. The height of the mountain above the sea over the middle point is there seen to be 2839 metres, and the altitude of the tunnel itself 1127 metres. The temperature of the rock in the tunnel is 30°·8 C., and of the ground, at the summit of the mountain above, 0° C. Hence the rate is $\frac{30 \cdot 8}{1712}$ degree C. per metre, which is nearly $\frac{1}{102}$ degree F. per foot. We will first assume the rate at the place A at the sea-level to be $\frac{1}{51}$ degree F. per foot, and the mean surface temperature there to be 50° F. Also if we take, as we have previously done, the ratio of $\rho : \sigma$ as that of granite to basalt, viz., 2·68 : 2·96, we find that $\frac{\sigma}{\sigma - \rho} = 10 \cdot 57$. Then, expressing the altitudes in feet, we have for the values of our symbols,

$$r = \frac{1}{51}, \; r' = \frac{1}{102},$$

[1] "Étude de l'Influence de la Chaleur de l'Intérieur de la Terre, &c." p. 7. See note, p. 154.

$$\frac{r'}{r-r'}=1, \quad \frac{\sigma}{\sigma-\rho}=10{\cdot}57, \quad h=9315,$$

$$b-b'=18, \ \frac{1}{r-r'}=102.$$

The thickness of the crust at the place A then comes out,

$$c=96623 \text{ feet}=18{\cdot}3 \text{ miles.}$$

And for the melting temperature,

$$\begin{aligned} a &= cr+b \\ &= 1944^{\circ} \text{ F.} \end{aligned}$$

We also find for the thickness of the crust at the mountain,

$$\begin{aligned} c' &= \frac{a-b'}{r'} = 1913 \times 102 \text{ feet} \\ &= 36{\cdot}9 \text{ miles,} \end{aligned}$$

If however we assume $\frac{1}{60}$ degree F. per foot as the rate of increment of temperature at the place on the sea-level, instead of $\frac{1}{51}$, we then obtain for the thickness of the crust there,

$$\begin{aligned} c &= 138032 \text{ feet} \\ &= 26{\cdot}1 \text{ miles.} \end{aligned}$$

And for the melting temperature

$$a=2350^{\circ} \text{ F.}$$

And for the thickness of the crust at the mountain

$$\begin{aligned} c' &= 235734 \text{ feet} \\ &= 44{\cdot}6 \text{ miles.} \end{aligned}$$

The observations which were made upon the temperature of the rocks in the Mont Cenis[1] tunnel, appear to have been less systematic than in the tunnel through Mont S. Gothard. The highest temperature recorded was 85° F. at a distance of 21000 feet from the southern opening, while the whole length of the tunnel was 40094 feet; so that that temperature occurred at about the middle of the tunnel. At that point the elevation of the tunnel above the sea was about 4391 feet, and

[1] "Nature," Vol. IV. p. 36, p. 415, and p. 434.

the greatest height of the mass of the Alps over the tunnel was 5307 feet. If we assume the temperature of the rock at the surface to be the freezing temperature, as in the case of S. Gothard, the above data give $r' = \frac{1}{100}$. And if we take the rate at a place on the sea-level to be $\frac{1}{51}$, and the temperature there 50° F., these data, when introduced into our equation, give

$$c = 20{\cdot}2 \text{ miles},$$

and a the melting temperature = 2141° F.

But if we take $\frac{1}{60}$ as the rate at a place on the sea-level, then

$$c = 29{\cdot}1 \text{ miles},$$

and $$a = 2612^\circ \text{ F.}$$

The rates of increase of temperature have been found to vary so much in different localities, that the rates at these two tunnels, though so nearly the same (and yet they are distant more than 130 miles), may differ somewhat from what might prove to be the average, were a greater number of mountains to be pierced. We may however feel assured that the rates determined represent pretty accurately the true rates at those places, because the amount of cover is so great, that the average is obtained for a great depth; a depth greater than has been reached in almost any other artificial excavation[1].

The results obtained in the present Chapter are decidedly corroborative of the views put forward in the tenth Chapter respecting the configuration of a section across a disturbed tract. For it is not easy to see on what hypothesis the conclusion regarding the downward convexity of the isogeotherms beneath a mountain can be explained, except on that of a protuberance of unmelted into melted matter. Indeed the existence of such roots to the mountain appears to be proved even more fully by the argument from temperature, than by that from geodesy.

Again, the melting temperature at which we have arrived, without making any assumption about the temperature at which the rocks would melt, agrees very nearly with that of cast iron, which is about that which has been assumed by the older

[1] An Artesian well is said to have been carried down to the depth of 5500 feet at Potsdam, Missouri. "Investor's Guide," Feb. 1881.

geologists as the probable temperature below the solid crust, whence the usual estimate for the thickness of the crust has been derived of about 20 miles.

But there is reason to think that the crust may be somewhat thicker, and consequently the temperature somewhat higher than we have just estimated them, even on the showing of our own formulæ, and that for the following reason. The value of c in equation (4) is increased if the ratio of $\sigma : \rho$ is made more nearly one of equality. We have taken this ratio to be that of basalt to granite, viz. 2·96 : 2·68; and the reason why we have done so is because the denser eruptive rocks are of the basaltic type, so that we may reasonably infer that the heavier fluid substratum is, as regards mineral character, a basic magma. But it by no means follows that, while not yet erupted, it should be as dense as basalt. Indeed the probability is in the other direction; because if, as we believe, it is in a condition of igneo-aqueous solution, its high temperature may diminish its density. Whether the water-substance combined with it will have such an effect will be hereafter discussed[1].

There is yet another reason, though perhaps a less cogent one, to lead us to suppose that we are taking the density of the substratum too great, if we assume it equal to that of basalt; for it has been already pointed out[2] that, according to Laplace's law, the density of basalt would be found at the depth of 100 miles, which is much more than that, at which we should expect the temperature of fusion to commence. It seems therefore almost certain that the value of σ is nearer to that of ρ than it has been assumed to be, and the resulting value for the thickness of the crust greater, and the temperature higher than those found; while the downward protuberance below the mountains will be also greater than estimated.

It is probable that the water, which is believed to be present, will render the rocks fusible at a lower temperature, and increase the liquidity of the magma.

Again we see, from the difference in the results for the thickness and melting temperature according as we take $\frac{1}{51}$ or

[1] p. 190. [2] See note, p. 118.

11—2

$\frac{1}{60}$ for the average rate, that the question we are dealing with is a delicate one, at the same time depending upon data about which we have no very precise knowledge. The thickness may therefore differ from the above results by a few miles, and the temperature by a few degrees either way. But if we are led to seek relief from the somewhat unexpectedly small values, which we have obtained by using the rate $\frac{1}{51}$, for the thickness of the crust, (because it will be recollected we have previously assumed it at 25 miles[1]) and of the melting temperature, it seems on the whole that it is to be found rather in assuming a lesser density for the substratum, than in assuming the lower value $\frac{1}{60}$ for the temperature rate[2]. But with every allowance made for some uncertainty, it must be admitted that the results at which we have arrived are singularly near what on other grounds we may expect to be the truth, while no gratuitous assumption has to be made in obtaining them.

It is also not unimportant that the two lines of argument in the preceding and present chapters, so diverse in their characters and yet pointing in a like direction, are drawn from observations made in regions as far apart as the Himalayas and the Alps; the conclusion from both being the same, namely that there is a protrusion of the lighter material of the crust into the denser subjacent fluid beneath the mountains in both these regions.

If we assume that the relative densities of the crust and subjacent fluid continue the same beneath the ocean as elsewhere, a reference to the right-hand side of the figure gives for k, the least thickness of the crust, which in this case will be beneath the ocean, because it is everywhere thickened beneath the continents,

$$k = c - \delta - \left(\delta - \frac{\delta}{\rho}\right)\frac{\rho}{\sigma - \rho}$$

$$= c - \frac{\sigma - 1}{\sigma - \rho}\,\delta.$$

[1] Chap. X. p. 126.

[2] For reasons for supposing $\frac{1}{51}$ to be rather below than above the average rate, see p. 16.

With the values hitherto used for σ and ρ,

$$k = c - 7\delta.$$

If we now give to δ any value approaching to that of the mean depth of the ocean, this will make the mean thickness of the crust very much less than it can be in fact. Indeed if c be less than about 25 miles, with the usual value 3·5 for δ, k would be negative.

It is evident therefore that this assumption cannot be correct, and that the relative densities of the crust and fluid beneath the oceanic areas cannot be the same as beneath the continents. We must therefore examine this point.

Now it is certain that, whatever be the form or law of density of the nucleus of the earth which is believed to be solid, in any fluid strata which may occur above it, surfaces of equal density will be also surfaces of equal pressure; for this is always true of a fluid in equilibrium under the action of such forces as exist in nature. This therefore must be the case within the fluid substratum. Let us then suppose that the depth measured from the bottom of the crust to such a *couche de niveau*, not far below the place A at the sea-level, is x, and that the depth of the same couche below the bottom of the crust under the ocean, where the crust has the mean thickness, is x'. And because the surface is one of equal pressure it will be also one of equal density, and σ will be the same at both these places. But the crust, being solid, may differ in density at the two places. Let its density then be ρ' below the ocean, where its thickness has the mean value k.

The surface of the ocean is likewise a *couche de niveau*. In order therefore that the hydrostatic pressure may be the same at the two places, taking the density of the ocean as unity, we must have, considering gravity constant for a depth so small compared to the radius,

$$\delta + \rho' k + \sigma x' = \rho c + \sigma x.$$

But these *couches de niveau* may be considered parallel,

$$\therefore \quad \delta + k + x' = c + x,$$

whence
$$(\sigma - 1)\delta + (\sigma - \rho')k = (\sigma - \rho)c \text{............} (1).$$

This is the general relation upon our view of the constitution of the outer parts of the globe, and suffices to explain the collection of water in the oceanic areas, and it shews that the crust beneath the ocean must be more dense than beneath the continents, for if we make $\rho' = \rho$ we get for k the value on p. 164 which makes it much less than it can possibly be. This greater density is in accordance with observations made with the pendulum, by which it has been found that gravity has a larger value at oceanic stations.

In order that the crust may not sink into the fluid we must have ρ' less than σ. If then we put $\rho' = \sigma$ in (1), this gives a limiting value for c and it is

$$c = \frac{\sigma - 1}{\sigma - \rho}\,\delta.$$

With the values previously assumed this gives

$$c = \frac{6{\cdot}86}{0{\cdot}28} = 24{\cdot}5.$$

In this case k is of course indeterminate, for if the crust is of the same density as the fluid its position of equilibrium will be independent of its thickness.

With the same values for ρ and σ we may write

$$(2{\cdot}96 - \rho')\,k = 0{\cdot}28c - 6{\cdot}86.$$

This is the equation to a rectangular hyperbola, in which the abscissa is the difference of the densities of the fluid substratum and the sub-oceanic crust, and the ordinate the thickness of that crust.

Since ρ' cannot be greater than σ or than, as assumed, $2{\cdot}96$, $(\sigma - \rho)c$ must be greater than $(\sigma - 1)\,\delta$; or, as assumed,

$$0{\cdot}28c > 6{\cdot}86,$$
$$\therefore\ c > 24{\cdot}5.$$

Or the thickness of the crust at the sea-level is not less than about 25 miles. It will be noticed that this is the first time, as far as the author is aware, that any estimate whatever regarding the thickness of the crust has been arrived at entirely independent of considerations of temperature. It is remarkable

how well it agrees with the others previously made, in which those considerations are involved.

Neither can ρ' be less than ρ (or as assumed than 2·68). It must therefore have some value between this and σ (or as assumed 2·96). Now if we suppose the sub-oceanic crust to consist of basic crystalline rocks, their density approaches very nearly to that of the basic eruptive rocks, and the probability is that $\sigma - \rho'$ is very small, and consequently k need not be small. But we have reason to believe it to be smaller than c, because at the locality A, which is continental, the crust has been probably thickened by compression. Let us then assume $c = 25$ miles and $k = 20$ miles.

We then obtain $\rho' = 2{\cdot}953$, a value which would admit of the sub-oceanic crust floating. This result is not unsatisfactory considering that we are dealing with densities about which we have no certain knowledge. It is probable that the absolute values of all the densities have been taken somewhat too high.

It is impossible to attain accuracy in such estimates as we have been engaged in making, but we may conclude as the result of our inquiries, that the crust of the earth at a place near the sea-level is about 25 miles thick; that beneath the central parts of the ocean it is about 20 miles thick, or perhaps less, and that it is more dense in those regions; and that it is by such increased density of the crust itself that the collection of water is to be explained, and not by increased density in the more deeply seated matter.

CHAPTER XIII.

AMOUNT OF COMPRESSION.

Our ideas respecting the sub-oceanic crust necessarily speculative.—Compression possibly confined to continental areas.—Compression might arise from extravasation of matter from beneath the crust, or from expansion of the crust.—Amount of compression needs to be estimated afresh.—Datum-level equation transformed to mean level.—Formula to express compression in terms of inequalities.—Estimate of inequalities from Atlas—(1) *on supposition that Oceans are due to compression,* (2) *that they are due to denser crust.*

THE great problem of the Physics of the Earth's Crust is, how to account for the compression which geology shows to have affected, at one time or another, almost every portion of the area of the surface with which we are acquainted. And this latter expression itself suggests a large and difficult question. To what extent are we acquainted with more than the land surfaces of the globe? It is hardly too much to affirm that our ideas respecting the nature of the earth's crust beneath the oceans are entirely derived by speculation from geology. We cannot explore it directly; but we know that most of the strata of the exposed portions of the surface have been deposited beneath seas, and from them we reason perhaps wrongly by analogy concerning the rocks at present composing the entire ocean floor. For it is the conviction of a large number of geologists[1], whose judgment is of weight on such a point, that the continents have always occupied the positions which they now occupy: by which it must be understood, that

[1] See a resumé of the history of this theory in a short letter by Dana, "Nature," vol. XXIII., p. 410.

they have oscillated about their present positions, sometimes more extended on one side and sometimes on another; but that the great oceanic areas and the great continental areas have never, within times to which geological records go back, interchanged places. If this be a true statement of the case, then it may be asserted that we know nothing about the geological constitution of the earth's crust beneath the great oceans. We can affirm from observation that all land surfaces have been more or less subjected to compression, but we cannot affirm with any certainty that the same has been the case with the ocean bottoms. We appear in the present state of our knowledge to be free to speculate upon this point. It is possible that the compression, which is so striking a feature in all true mountain ranges, and is by no means confined to them, may be a continental phenomenon only; that is, may belong only to continents, and to islands which are geologically remnants of continents. The results at which we have arrived at the end of the last chapter tend to confirm this view, and to throw more than a doubt upon the doctrine that the abnormal elevation, or radial excess, of the surface of dry land above the ocean floor is due altogether to compression. We need not however decide at once upon this question, but, using general symbols in our reasoning, leave it for the present open.

There are two ways in which compression may have acted to elevate the crust, supposed to rest on a fluid substratum. In the one the anticlinals would form ridges, whose sections would be cusp-like and the subjacent fluid would rise into them. This hypothesis has been discussed in the ninth chapter, and has been shown not to accord with natural appearances. It is one however which has been more or less assumed in many geological writings[1]. The other is that developed in the tenth chapter, and shown in the eleventh and twelfth to be capable of explaining two classes of phenomena perfectly independent of each other, but which, it may be observed, would be absolutely reversed were the doctrine true, that the heavier and hot molten liquid rises into the anticlinals.

[1] See Prof. A. de Lapparent, "L'Origine des inégalités de la Surface du Globe." *Revue des Questions Scientifics.* Juillet, 1880.

We have then to seek for the cause of this compression, which has effected the continental areas.

If we have given a sphere of a certain radius, whose outer crust is solid and rests upon a fluid substratum, compression of the crust may arise from contraction of the volume of the sphere, either through cooling, or by the extravasation of some portion of the matter which was originally beneath the crust. In these cases the compression of the upper surface of the crust would be equal to the contraction of the sphere; that is to say, if the radius of the sphere before contraction was $r+er$, and after contraction became r, e being the coefficient of contraction, then the mean coefficient of compression along any line drawn upon the sphere would also be e. If the length of the line after compression was l, then before compression it must have been $l+el$.

But there may be another cause for the compression which has elevated continental areas. The solid crust may have expanded horizontally. It is quite possible that these two causes of compression may have coexisted. The sphere may have contracted, and the crust expanded simultaneously; in which case the contraction of the volume of the sphere will be no measure of the compression of the crust.

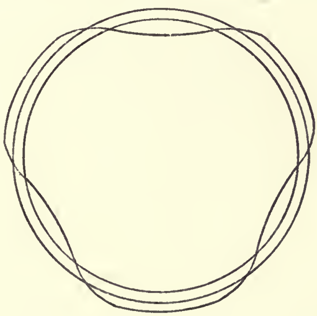
We have already made it apparent, that the cooling of the earth considered as a solid sphere cannot account for the inequalities, and therefore neither sufficiently for the compression of the crust. And if we regard the crust as resting on a liquid substratum, it does not appear how its contraction through mere cooling can have been so much greater than in the case of a solid globe, as to account for what the latter will not explain. We have therefore to examine whether other hypotheses will more satisfactorily explain the phenomena. But it is obvious that our first step, before we can reason at all about the compression under the condition of a fluid substratum, must be to estimate its amount afresh: for the grounds upon which that must be done are very different from those upon which we formerly estimated the compression in the case of a solid globe[1].

[1] See chap. v., p. 55.

If we revert to the datum level equation (1) in the fifth chapter[1], which is perfectly general for a vertical section of the crust under compression, and adapt it to volumes, as has been done in equation (3)[2], but retaining terms answering for volumes to $\Sigma\,(\alpha)$ and $\Sigma\,(\beta)$ in equation (1), which terms we will call $\Sigma\,(X)$ and $\Sigma\,(Y)$, we then have,

$$8\pi r^2 ke = \Sigma\,(A) - \Sigma\,(B) + \Sigma\,(Y) - \Sigma\,(X),$$

for our general datum-level equation of volumes for a solid crust resting on a fluid substratum. And we know that in this expression $\Sigma(Y) - \Sigma(X) = 0$. In this equation it will be recollected that e is the mean linear compression of the crust, and that the surface above which the volumes $\Sigma\,(A)$, and below which the volumes $\Sigma\,(B)$, are reckoned, is the imaginary surface which occupies the position that the upper surface of the crust would occupy at the present time, had it been perfectly compressible in a horizontal direction; and that $\Sigma(X)$ and $\Sigma(Y)$ are the volumes similarly situated with respect to the lower surface of such a crust[3]. Let us now transform this equation to the upper and lower "mean levels" of the crust, which by the definition will be at the same distance apart as the datum levels[4].



In the same manner that A, B, X, Y are referred to the datum levels, let A', B', X', Y' be referred to the mean levels; and, in the figure, let in the first place the outer circle be the lower mean level, and the inner one the lower datum level;

[1] p. 47. [2] p. 50.
[3] p. 48. [4] p. 115.

and let the distance between the two circles, that is the distance between the lower mean and the lower datum level, be ϵr, where r is the radius of the sphere to the upper datum level. Then if k be the mean thickness of the crust,

$$4\pi\epsilon r\,(r-k)^2$$

is very nearly the volume of the shell between these two levels. And so it is easily seen from the figure, that

$$4\pi\epsilon r\,(r-k)^2 = \Sigma\,(X) - \Sigma\,(X') + \Sigma\,(Y') - \Sigma\,(Y).$$

But we know that, by the datum level equation,

$$\Sigma\,(X) - \Sigma\,(Y) = 0\,[1];$$

$$\therefore\quad 4\pi\epsilon r\,(r-k)^2 = \Sigma\,(Y') - \Sigma\,(X'),$$

and

$$\epsilon = \frac{\Sigma\,(Y') - \Sigma\,(X')}{4\pi r\,(r-k)^2}\,.$$

Secondly, suppose the outer circle to be the upper mean level, and the inner one the upper datum level. Then the volume of the shell between these two levels is nearly $4\pi r^2\epsilon r$.

And from the figure,

$$\Sigma\,(A) - \Sigma\,(A') + \Sigma\,(B') - \Sigma\,(B) = 4\pi r^2\epsilon r\,;$$

$$\therefore\quad \Sigma\,(A) - \Sigma\,(B) = 4\pi r^2\epsilon r + \Sigma\,(A') - \Sigma\,(B') \ldots\ldots\ldots (\text{A}).$$

But, by the datum level equation,

$$\Sigma\,(A) - \Sigma\,(B) = 8\pi r^2 ke.$$

And the value of ϵ has just now been found.

Hence, substituting for $\Sigma\,(A) - \Sigma\,(B)$ and ϵ in (A),

$$8\pi r^2 ke = \frac{r^2}{(r-k)^2}\{\Sigma(Y') - \Sigma(X')\} + \Sigma(A') - \Sigma(B') \ldots (\text{B}).$$

We suppose for the present that the crust beneath the oceans is of the same density as elsewhere, and attribute their presence to depressions of the surface, owing to the crust being thinner beneath them. This is the particular case in the last chapter[2] when ρ' is put equal to ρ.

[1] This follows from the relation $\Sigma\,(\alpha) = \Sigma\,(\beta)$; p. 47.

[2] p. 164.

Again, for a section across a disturbed tract of unit of width we have the two relations[1],

$$\Sigma(a) - \Sigma(b) = \frac{\sigma - \rho}{\sigma} kle + \frac{\mu}{\sigma}\{\delta l - \Sigma(d)\} \ldots\ldots\ldots(1),$$

and

$$\Sigma(\beta) - \Sigma(\alpha) = \frac{\rho}{\sigma} kle - \frac{\mu}{\sigma}\{\delta l - \Sigma(d)\} \ldots\ldots\ldots\ldots\ldots(2).$$

If instead of a section of unit of width we substitute a rectangular area of length l, and width w, these equations become[2]

$$\Sigma(A') - \Sigma(B') = \frac{\sigma - \rho}{\sigma} 2klwe + \frac{\mu}{\sigma}\{\delta lw - \Sigma(D)\},$$

and

$$\Sigma(Y') - \Sigma(X') = \frac{\rho}{\sigma} 2klwe - \frac{\mu}{\sigma}\{\delta lw - \Sigma(D)\}.$$

As has been shown for $\delta l - \Sigma(d)$ in the case of unit of area[3], so it is evident here that $\delta lw - \Sigma(D)$ is the water displaced by rock owing to the elevation; l and w being the length and width of the disturbed tract, and $\Sigma(D)$ the volume of any water which may happen to lie over it.

We have then the proportion

$$\frac{\Sigma(A') - \Sigma(B') - \frac{\mu}{\sigma}\{\delta lw - \Sigma(D)\}}{\Sigma(Y') - \Sigma(X') + \frac{\mu}{\sigma}\{\delta lw - \Sigma(D)\}} = \frac{\sigma - \rho}{\rho} \ldots\ldots\ldots(3).$$

Now it is evident that the same proportion will hold for the area of the whole globe. In this case the "tract" becomes the entire area (for there is nothing in our reasoning to confine the relations (1), (2), to any particular portions of it whether disturbed or not). $\Sigma(D)$ then becomes the whole ocean; $\delta lw - \Sigma(D)$ becomes the water displaced by all the elevated regions, and we will assume this to be equal to $L\delta$, where L is the area of the land; the reasons for which assumption will appear by and bye.

[1] Pp. 116, 117.

[2] See chap. v., p. 49.

[3] See chap. x., p. 116.

Making this substitution we obtain from (3),

$$\Sigma(Y') - \Sigma(X') = \frac{\rho}{\sigma - \rho}\{\Sigma(A') - \Sigma(B')\} - \frac{\mu}{\sigma - \rho} L\delta.$$

And substituting this value in (B),

$$8\pi r^2 ke = \frac{r^2}{(r-k)^2}\left[\frac{\rho}{\sigma-\rho}\{\Sigma(A') - \Sigma(B')\} - \frac{\mu}{\sigma-\rho} L\delta\right] + \Sigma(A') - \Sigma(B');$$

whence

$$e = \left\{\frac{\rho}{\sigma-\rho}\frac{r^2}{(r-k)^2} + 1\right\}\frac{\Sigma(A') - \Sigma(B')}{8\pi r^2 k} - \frac{r^2}{(r-k)^2}\frac{\mu}{\sigma-\rho}\frac{L\delta}{8\pi r^2 k}.$$

If we can estimate $\Sigma(A') - \Sigma(B')$ and $L\delta$ we shall now be able to find e for any assumed value of k.

Let us pause to consider what the symbols involved import. If we regard the compression to arise solely from diminution of volume beneath the cooled crust, and not from the extension of the crust itself, then e is the mean coefficient for the compression of the whole crust, which has arisen from this cause since the existing inequalities began to be formed. If however we neglect the contraction of the matter of the crust itself, which for a thickness of 20 miles or so the results of Chapter VI show that we may do, then the coefficient of compression for the crust will be the same as that of contraction for the sphere. We do not know exactly the laws which govern, either the contraction of the matter of the crust, nor yet those which determine the rate at which it thickens through solidification. But it is evident that the compression of the surface must be greater than the mean compression.

For the remaining symbols r is the present radius, k is the mean thickness of the crust, considered of equable density, that is to say, it is the thickness which the crust would have, if it had been perfectly compressible, so that the contraction of the globe should not have caused it to be wrinkled or thickened anywhere.

Consequently, any thickening from sedimentation is not included in k; for that will be derived from matter elevated by compression. $\Sigma(A')$ and $\Sigma(B')$ are the volumes of the elevations and depressions above and below the upper mean level of this crust; the position of the crust as a whole being affected by the lateral flow of the subjacent fluid, through the intrusion into it of the downward protuberance of the mountain roots. This is allowed for in the equation as if the fluid were a perfect fluid.

If we assign to σ the value infinity in this expression it ought to reduce to the datum-level equation, for in that case there would be no downward protuberances, and therefore the mean level of the crust would not be altered from the datum-level, because there would be no flow of fluid beneath it. Now this it does, for when $\sigma = \infty$ the equation becomes

$$8\pi r^2 ke = \Sigma(A') - \Sigma(B'),$$

which is the datum-level equation.

It is self-evident that it will be quite impracticable to arrive at anything approaching to an accurate estimate for $\Sigma(A') - \Sigma(B')$, because, in the first place, we can only guess at the position of the mean level; and secondly, we can only roughly estimate the amounts of $\Sigma(A')$ and $\Sigma(B')$ respectively. Nevertheless it will appear that we may draw important conclusions even from such guesses as we can form.

Referring to Atlases[1] which have been published since the voyage of the *Challenger*, we find the oceanic area divided into five portions, whose mean depths are taken to be,

(1) from 1 to 2 miles,
(2) from 2 to 3 miles,
(3) from 3 to 4 miles, $(= \delta)$,
(4) from 4 to 5 miles,
(5) above 5 miles.

The first may be taken as extensions of the elevations which have produced the continents. The second, where not connected

[1] For example, "Letts's popular Atlas" maps 3 and 4. See also "Thalassa" by J. J. Wild, Ch. I. Marcus Ward, 1877.

with and prolongations of the first, may be considered as submarine elevations, and it is possible that they may be the remains of ancient lands levelled down. The third which occupy the larger portion of the oceanic area we will regard as indicating the mean upper level of the crust. The fourth and fifth will then be depressions below it, corresponding to the series $\Sigma(B')$. If then we set the fourth and fifth against the first and second, we shall have remaining for the excess of $\Sigma(A')$ over $\Sigma(B')$ only the volume of the continents above the mean level. If we then adopt Prof. Haughton's estimates[1] for L the area of the land, and for its mean height h above the sea, δ being the depth of the sea over the area which we take as defining the mean level, we shall have,

$$L = 52 \times 10^6 \text{ square miles,}$$

$$h = 1000 \text{ feet} = 0{\cdot}19 \text{ mile nearly,}$$

$$\delta = 3{\cdot}5 \text{ miles.}$$

Then $\Sigma(A') - \Sigma(B') = 52 \times 10^6 (3{\cdot}5 + 0{\cdot}19)$ cubic miles,

$$= 52 \times 3{\cdot}69 \times 10^6 \text{ cubic miles.}$$

If we put $r = 4000$ miles,

$$k = 20 \text{ miles,}$$

$$\therefore \frac{r^2}{(r-k)^2} = 1{\cdot}01,$$

and $$\frac{\rho}{\sigma - \rho} = 9{\cdot}57,$$

and introduce these values into the expression for e, we find for the first term, 0·254.

As regards the second term, it depends upon the water displaced by rock owing to the elevation. Now we have, in the above classification of the ocean depths, Nos. (1) and (2) which are less than the mean, and therefore cover upraised crust. We have also Nos. (4) and (5) which are greater than the mean, and therefore cover depressed crust. All these belong to the disturbed regions, and consequently we may roughly set them off against one another.

[1] p. 55.

We then shall have $\delta lw - \Sigma(D)$, or the water displaced, equal to a layer of the area of the land, and of the mean depth of the ocean, or equal to $L\delta$, as already assumed.

To calculate the second term, we may assume

$$\left.\begin{array}{l}\mu = 1 \\ \sigma = 2{\cdot}96 \\ \rho = 2{\cdot}68\end{array}\right\}, \quad \text{and} \quad \frac{L}{4\pi r^2} = \frac{52}{197}\,[1],$$

and we obtain 0·083;

whence
$$e = 0{\cdot}254 - 0{\cdot}083,$$
$$= 0{\cdot}171.$$

The contraction indicated by this result is greater than can be admitted; remembering that e is the mean compression for the whole thickness of the crust, and that the contraction of the outer surface of the globe, and therefore of its radius, is probably nearly the same, it would require that the radius of the earth should have been shortened by nearly 700 miles since a crust began to be formed. If we suppose the substratum of lesser density as compared with the crust than the density of basalt as compared with that of granite, then, owing to the smaller value of $\sigma - \rho$, this contraction would have to be put higher. Nevertheless on our conclusions regarding a fluid substratum, we cannot put the ratio $\frac{\rho}{\sigma - \rho}$ at a less estimate than we have done; for even that is probably too small, since it is not likely that the density of the substratum should not be lessened both by its exalted temperature, and possibly by the combined water; so that σ will be more nearly equal to ρ than the density of basalt is to that of granite. Our hypothesis must therefore be in fault.

Now in obtaining the above estimate, we have taken the crust to be of the same density beneath the oceans as beneath the continents. But we have shown[2] that that cannot be the case, for that it must be there more dense, and that the equilibrium of the ocean is due to that circumstance. We must

[1] See ch. v., p. 55, note. [2] ch. XII., p. 165.

therefore admit that, if the continental areas have been gradually elevated by compression out of sub-oceanic crust, a diminution of density must have been a consequence of their elevation. For this we shall attempt to account hereafter.

If we suppose that, instead of the existing denser crust beneath the ocean, we substitute a crust of the same thickness as at the sea-board, and of the density ρ, and omit the consideration of the ocean, then the compression of such a crust parallel to the direction of its surface (which would be the same as the existing sea-level) would, on a section of unit of width, produce elevations of the mass ρkle for the compression e. And on the supposition that these occurred in the same areas that the existing elevations do occur, their mass would be the same as that of the existing elevations. The radius to the surface of such a crust would be the same as to the surface of the existing ocean. We are therefore at liberty to make use of this suppositious case for the purpose of obtaining an approximate value for the compression, in order to avoid the difficulties which would occur in reasoning about a crust of density differing in different regions.

Reverting therefore to our expression for the value of e, it is evident that the second term must be omitted, for that depends upon the amount of water lying upon the surface of the disturbed area. It reduces therefore to

$$e = \left(\frac{\rho}{\sigma - \rho}\frac{r^2}{(r-k)^2} + 1\right)\frac{\Sigma(A') - \Sigma(B')}{8\pi r^2 k}.$$

Next in estimating $\Sigma(A') - \Sigma(B')$ we must omit the consideration of the elevation of the sea-level above the mean level of the crust beneath the ocean, using the surface of the ocean as if it were the mean level. Consequently the part involving δ, or 3·5, will disappear, and we have

$$\Sigma(A') - \Sigma(B') = 52 \times 10^6 \times 0{\cdot}19 \text{ cubic miles.}$$

We must also use for k the value of the thickness at the sea-level, which we put at 25 miles. If we now substitute the values which we have been using for ρ and σ, we get for the mean coefficient of compression

$$e = 0{\cdot}0105.$$

This would make the radial contraction of the sphere, supposing the compression to be caused by the contraction of the matter beneath the crust, about 42 miles.

Lastly, if we regard compression as a continental phenomenon only, since the area of the land is $\frac{52}{197}$ths of the whole area of the globe, the compression of this would be

$$\left(\frac{197}{52}\right)^{\frac{1}{2}} \times 0{\cdot}0105 \text{ or } 0{\cdot}0204,$$

or about 2 per cent.

The estimates we have now been making, inexact as they must be allowed to be, nevertheless go far to confirm our previous conclusion that the ocean basins are not caused by depressions in the upper surface of a crust of the same density as that beneath the continents, but that they are due to a greater density and general depression of the sub-oceanic crust.

CHAPTER XIV.

EXTRAVASATION OF WATER WILL NOT ACCOUNT FOR COMPRESSION.

Hypothesis that oceans consist of water that has been extravasated.—Density of the water before extravasation—volume of interior which would need to have contributed it.—Hypothesis shown to be inadequate to produce the entire compression.—Disturbance of crust by change of ellipticity of spheroid.—Inequalities do not appear connected with such a cause.

In a former part of this book[1] we have suggested the extravasation of water substance from beneath the crust as a possible cause of contraction, and of the consequent compression, of which we are in quest[2]; and have proposed to examine its validity[3]. This we will now proceed to do, availing ourselves of the approximate values for the compression obtaining in the preceding chapter.

We have already pointed out how large a portion of water is emitted in volcanic eruptions, and submitted a hypothesis to account for its presence beneath the crust[1]. It is certain that it immediately falls in rain and goes to increase the volume of the ocean. If then we put the ocean out of the question, and consider the configuration of the solid surface of the earth, we have before us, on the present hypothesis, the surface of a globe, which has been compressed solely by the contraction of the globe, caused by the extravasation of a certain portion of its original internal substance. This then falls under the case, where the coefficient of contraction will be the same as that of compression.

[1] p. 88. See also Judd's "Volcanoes," fig. 5. London: Kegan Paul and Co., 1881.

[2] See chap. VIII.

[3] See p. 90.

In order to estimate the contraction of the globe as a whole, which would arise from the extravasation of a certain quantity of water-substance from beneath the crust, we may proceed as follows.

Suppose s to be the volume of the water-substance in question mingled with the rocky matter in the magma, and we will assume that when s is extravasated the volume of the magma is diminished by s. This assumption is most favourable to the diminution of volume, but excessive; because, if the rocky matter of the magma be in solution under pressure along with the superheated water, the volume of the combined substances is probably less than what the sum of the two volumes would be separately.

Let μ be the density of the water substance. Then μs is its mass. Now if this goes to form a layer of water of depth δ' and density 1 over the whole globe, the mass of such a layer will be $4\pi r^2\delta'$ very nearly, because δ' is very small compared with r.

Hence $$\mu s = 4\pi r^2 \delta'.$$

Now the radius of the sphere before the extravasation of this water substance having been $r + er$, the diminution of volume caused thereby will be $\frac{4}{3}\pi\{(r+er)^3 - r^3\}$, and this must be equal to s; that is to

$$\frac{4\pi r^2\delta'}{\mu},$$

$$\therefore\ \frac{\delta'}{\mu} = r\left(\frac{e^3}{3} + e^2 + e\right).$$

$$\text{And}\quad \mu = \frac{\delta'}{r\left(\frac{e^3}{3} + e^2 + e\right)}.$$

Putting the area of the ocean at 145 millions of miles, the area of the globe being 197 millions, it is obvious that

$$\delta' = \frac{145}{197}\,\delta,$$

δ being the mean depth of the ocean, and $= 3{\cdot}5$ miles.

Consequently, with the value 0·0105 just found for e,

$$\mu = 0{\cdot}061.$$

If we know how much the volume of a given existing volume of the interior has been diminished by the abstraction of the water it contained, we can find the depth to which the loss of water has extended.

For instance, if V be the volume of a mass of the interior at present, after the extravasation of the volume V' of water substance at the density μ which was previously mingled with it, and if we consider the abstraction of the mass $\mu V'$ to diminish the volume of the magma by V', which is probably too much, but we may assume for our purpose as true, then the mass was originally $\sigma V + \mu V'$ and its density $\dfrac{\sigma V + \mu V'}{V + V'}$. Now let us assume that this density was at that time, before any water was extravasated and the crust had but just begun to be formed, equal to the density of the crust as it now exists beneath the ocean; for if it had been but little less the crust would have sunk;

$$\therefore \frac{\sigma V + \mu V'}{V + V'} = \rho',$$

ρ' being the density beneath the ocean;

whence
$$V = \frac{\rho' - \mu}{\sigma - \rho'} V'.$$

But V' is supposed to be equal to s, or to the volume which the ocean would have if all the water were reduced to the density μ;

$$\therefore V = \frac{\rho' - \mu}{\sigma - \rho'} \frac{4\pi r^2 \delta'}{\mu}.$$

This is the present reduced volume of the portion of the interior, which must have formerly had all the water of the ocean mingled with it at the density μ, so as to have then reduced it from the present density μ to the then density ρ'.

We now wish to find what portion of the sphere is comprised within this volume. Suppose it to be a shell of thickness z situated immediately beneath the crust.

The volume of this shell will be

$$\frac{4\pi}{3}\{(r-k)^3-(r-k-z)^3\}.$$

And this must be equal to V, or to

$$4\pi\frac{\rho'-\mu}{\sigma-\rho'}\,\frac{r^2\delta'}{\mu};$$

whence, giving to ρ' the value 2·953[1],

$$z=\left\{3\frac{\rho'-\mu}{\sigma-\rho'}\,\frac{r^2\delta'}{\mu}-(r-k)^3\right\}^{\frac{1}{3}}+(r-k),$$

$$=9183+3980=13163 \text{ miles.}$$

The two other roots of the equation are impossible. Hence the entire remaining portion of the sphere beneath the crust could not have contributed sufficient contraction to have caused the requisite compression, even under circumstances more favourable than are likely to be true regarding the increase of volume due to the presence of the water.

From this result we feel obliged to abandon the hypothesis that the existing inequalities can be accounted for by the extravasation of the water, now composing the oceans, from beneath the crust.

There is one other cause which must necessarily have disturbed the equilibrium of the crust during the lapse of ages, and that is the diminution of the oblateness of the spheroid. The friction of the tides, whether oceanic or bodily, must necessarily have diminished the rotational velocity, and lessened the oblateness. The parts of the crust about the poles will consequently have been subjected to stretching, and those about the equator to compression. There is however no apparent reason immediately to connect the inequalities with this cause, for the continents do not occupy an equatorial belt, as they would do under this hypothesis, nor have the polar regions been free from the compression which all continental areas have experienced.

[1] See p. 167.

If however a radial contraction of 42 miles would have been sufficient to have produced the whole of the existing inequalities, it is difficult to understand how the cause now alluded to can have been without appreciable effect. We are therefore tempted to suggest, that the apparent want of relation between the areas of compression and the axis of rotation favours the idea, so strongly supported by phenomena of climatal changes, that the latitudes of places on the earth's surface are liable to change, a change which would be impossible were the earth solid, but which with a fluid substratum is brought within the range of possibilities.

CHAPTER XV.

COMPRESSION AND VOLCANIC ACTION.

Compression not being sufficiently accounted for by contraction of the globe, alternative hypothesis that the cause of it is situated within the crust—Attraction of thickened crust not operative—Intrusive dykes may afford a key to the problem—may be widened by fluid pressure—Excess of horizontal over vertical pressure so caused—Their connection with volcanic vents—Hypothesis concerning the constitution of the magma—mode of its elevation in a fissure—Pressure on the sides of the fissure—Comparison of the work of compression with that done against gravity—Mineral veins—Prof. Judd and Baron Richthofen on the relation of volcanic energy to continental movements—Possible additional compression upon solidification of dykes—Analogies of Earth's crust and ice—Prof. Nordenskiöld's theory of compression—Analogy of ice disturbed by skaters and Earth's crust by sedimentation—Corrugation attributed to cracks opening from below, *and a continental phenomenon—Views of American Geologists as set forth by Dr Sterry Hunt—Their observed facts agree with the present theory—Effect of increased thickness of crust on compressing force—Whence the energy invoked—Circulation of rock—Suggestion to explain diminished density of continental areas—"Red clay" of profound ocean.*

THUS far the compression, which is known to have affected many regions of the earth's surface, has been considered on three hypotheses, as arising (1) from a diminution of the volume of the sphere through cooling as a solid by conduction; (2) from the collapse of a thin flexible crust covering a contracting interior and resting upon a subsiding liquid substratum; and (3) from a general diminution of the interior beneath a solidified and partially rigid crust through the extravasation of water-substance from beneath it. But we have found that none of these hypotheses account sufficiently for the phenomena. We will now proceed to examine the alternative hypothesis, that the causes of compression of the crust are situated within the crust itself.

At first sight it might seem that, when the crust had been thickened in any region, the increase in the mass of the crust there through thickening might add to the attractive force of

that region, and cause compression, by drawing the neighbouring parts towards itself. But the discussion of the attraction of mountain masses by Sir G. B. Airy, quoted in the eleventh chapter, suffices to show that such a supposition would be erroneous. Indeed it is easy to see that, when a column floats vertically in a heavier fluid, the horizontal attraction upon a particle, at or near the surface of the fluid, will be somewhat less for a longer than for a shorter column. It is not possible therefore that the cause of compression can be explained by the attraction of the thickened crust upon the neighbouring parts.

To guide us in this new branch of enquiry, let us enquire whether any geological appearances in the crust itself can give a key to the cause of compression.

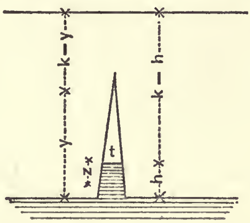
When we examine any district where considerable sections of metamorphosed strata are exposed, we are struck with the abundance of dykes of intrusive rocks, which permeate them[1]. These are commonly, though not always, more or less vertical in position. Likewise in any section, carried across an extensive range of country, large and wide dykes are laid down, sometimes as actually determined by the surveyors, and sometimes as the supposed channels, through which overlying sheets of igneous rock have been raised to the surface. What have been the conditions under which these intrusive more or less vertical sheets of matter have found their way into the positions in which we see them? That they have come up from below no one doubts; but what has been the force that has injected them? How have the chasms originated which they occupy? If the separation of their walls has been effected by any pressure acting within the chasms themselves, then we have an indication of the existence of some force tending to compress the crust.

Upon the hypothesis that a crust of about 25 miles thickness rests upon a fluid substratum of highly heated rocky matter in a state of igneo-aqueous fusion, let us suppose that a crack is

[1] Prof. Liveing, from an estimate by the eye alone, made at my request in the Channel Islands, thinks that the horizontal area occupied in the cliffs of Guernsey and Serk by intrusive dykes cannot be less than from 2 to 3 per cent. of the whole area. Jersey consists largely of volcanic products.

produced by any cause in the under surface of the crust. The pressure of the fluid at the bottom of the crust is that due to the weight of the superincumbent crust, and consequently the tension of the water-substance in the magma there must be the same. If therefore, upon the opening of a fissure which did not reach the upper surface, the magma beneath were able to fill it with water-substance, which we may provisionally call vapour[1], at a tension equal to the pressure which it sustains from the weight of the crust (about 10066 tons upon the square foot), the fused rock could not rise into the fissure at all, but the sides of the fissure would be subjected to a vapour pressure, only less than the pressure of the entire crust by the diminution upwards due to the decreasing weight of the superincumbent vapour, which, compared with its tension at the bottom of the chasm, would be inconsiderable. Thus we should under the circumstances supposed (which could never be fully realised) have a horizontal pressure upon every elementary area of the sides of the chasm equal to the weight of a column of rock of the same sectional area and about 25 miles high.

Generally, let t be the tension of the water-substance which occupies the chasm, h the height to which the magma rises in the chasm, then, if we neglect the weight of the water-substance



compared with that of the magma, which owing to its high temperature we are probably justified in doing,

$$t = g\rho k - g\sigma h.$$

This will be the horizontal pressure upon the side of the fissure everywhere above the surface of the fluid magma in the fissure,

[1] Perhaps it ought rather to be called gas. See Hannay "On the states of matter," *Proc. Royal Soc.*, Vol. XXXII. p. 412. 1881.

while the horizontal pressure at a depth z below the same surface will be

$$t' = g\rho k - g\sigma h + g\sigma z.$$

At the same time the vertical pressure upon an element of the rock in the crust near the chasm at a height y above the bottom of it will be

$$p = g\rho\,(k - y).$$

Hence the difference between the horizontal and vertical pressures on such an element situated above the level to which the magma rises in the chasm, will be

$$t - p = g\rho y - g\sigma h.$$

And the like difference below that level will be

$$t' - p = g\rho y - g\sigma h + g\sigma z.$$

The excess of the horizontal over the vertical pressure, which is the force that will cause lateral compression, increases as y increases, and also increases as h diminishes. This force will therefore be greater towards the upper part of the chasm, and it will also be greater the greater the tension of the vapour, which will tend to decrease h by preventing the magma rising. If the rock should allow the vapour only slowly to escape, and if the magma below the chasm can supply fresh vapour sufficiently rapidly to keep up the temperature, it is conceivable that the tension of the vapour in the chasm may be nearly equal to $g\rho k$, in which case h will be nearly evanescent, and the horizontal pressure much greater than the vertical especially in the upper part of the chasm where y is nearly equal to k. We can therefore understand how a chasm, commencing below, may be rent open upwards, and at last reach the surface of the ground. When that occurs, vapour at a high tension will escape at whatever point is first broken through, and after issuing at a diminishing pressure for a certain time, it will be followed by the magma itself, which will overflow at the surface because the water-substance in the upper part of the column, expanding owing to the diminished pressure there, will render the whole column of less weight than an equal column of crust. This appears to be the mechanism required to explain the formation of a volcano, and of the earthquakes which precede its appearance.

It is probably to this kind of action that we may attribute the opening of fissures radiating from volcanic vents, along which subsidiary craters are established. Thus for instance an earthquake, caused by a volcanic explosion in Iceland on the 4th January, 1875, "opened rifts forty miles in length in the plains north of Dýngjufjöll, and it was from fissures in a tract twenty miles in length and about three in breadth, which sank to some depth between two of these rifts, that the lava welled forth[1]."

If however the water-substance should not be able to escape from the magma sufficiently rapidly for the vapour pressure to accumulate so as to prevent the magma from almost entirely occupying the fissure, or if the fissure should open to the surface so that the vapour above the fluid should escape, then the magma would rise into the fissure pressed upwards by the weight of the crust around the base of the ascending column. In this case it would not behave within the fissure like an inert liquid, for it would carry with it the water-substance which is one of its constituents, and that, when raised within the fissure, would have an expansive force of its own. For this expansive force, although equalised at the bottom of the crust to the pressure of the crust, arises from the store of energy due to the temperature of the water-substance, and to whatever place it may be transferred it will carry that energy with it.

In order to come to any conclusion about the amount of this force we must make some supposition concerning the constitution of the magma, which for convenience we will call "lava" after it has risen into the fissure, retaining the name of magma for the fluid substratum.

The supposition which we will make shall be that the magma consists of hot rocky matter in combination with superheated water. In a given mass of the magma there is a certain proportion of rocky matter which is inelastic, and the remainder consists of water-substance at the same exalted temperature, the presence of which renders the magma elastic, and if the pressure were infinitely increased the whole would be reduced

[1] "Volcanic History of Iceland," by Mr W. G. Lock, *Geol. Mag.* Dec. II., Vol. VIII. p. 214.

to the volume of the rocky matter. We conceive this association between the two at an exalted temperature and under great pressure, which has been called igneo-aqueous solution, to be not an ordinary solution of the rock in the water, but rather of the water in the rock, for we cannot suppose that the small relative quantity of water which is present can hold the rock in solution as salt is held in solution in water. And the water perhaps acts in the manner of a flux rendering the rock fluid at a lower temperature than that at which it would be fusible without it. Prof. Judd compares the process with the power, known to be possessed by "various solids and liquids, of absorbing many times their volume of certain gases[1]." We know from the behaviour of lava in a volcanic vent that when the pressure is diminished the water separates from the rocky matter into vesicles which are at first of microscopic minuteness, but eventually these join one another, expand, and ascend to the surface, producing ebullition. Since then there is apparent this tendency for the water to separate when the pressure is diminished, and since when it does separate it rises through the lava, it seems a necessary conclusion that under the pressure of the crust the density of the magma is uniform, for if it were not so, the water would rise to the surface of it and form a layer upon it.

That there should be a layer of water-substance intercalated between the crust and the substratum is on many accounts in the highest degree improbable. We must therefore arrive at the conclusion, startling though it may appear, that the water-substance in the magma is reduced by solution under pressure to the same density as the rocky matter with which it is combined.

The fact that the water-substance separates from the lava when the pressure is diminished, leads us to conclude that the quantity of it which the magma is able to hold in solution at a given temperature must be dependent on the pressure. If then

[1] "Volcanoes," p. 345. See also a notice by Mr Hannay "On the absorption of gases by solids," *Proc. Royal Soc.*, Vol. XXXII. p. 407. 1881. His experiments appear to be in accordance with the assumptions here made.

a fissure should commence to be formed at any place in the bottom of the crust, some portion of the water-substance would escape from the magma into it, its place in the magma being partially supplied from the magma in the vicinity. By the continuation of this process the fissure would become filled by what we have called vapour, at the elastic pressure due to the temperature[1]. The case would be analogous to that of a liquid confined in the presence of its own vapour. The upper portion of the lava in the fissure would retain only so much water as the pressure of the vapour upon its surface would enable it to keep in solution. But at lower levels, where the pressure would be increased by the weight of the superincumbent lava, it would retain more water.

Suppose then that at the depth x below the surface of the crust the volume of a certain mass of lava is $V+v$, where V is the volume of the rocky matter which we suppose incompressible and v is the volume by which it is increased owing to the water-substance which is combined with it. We have no means of knowing what law connects v with the pressure p; but we feel assured that it diminishes as the pressure is increased. If we assume $v=\frac{\alpha}{p}$, α is probably some function of p; but for want of a better hypothesis we will suppose α constant, and then v becomes subject to Boyle's law. The rocky matter may itself contain water as one of its chemical constituents, which we leave out of the question. Let μ be the mean density of the mixture and β the mass under consideration. Then our hypothesis gives the relation

$$\mu=\frac{\beta}{V+\frac{\alpha}{p}}.$$

The temperature is taken as constant.

[1] Richthofen (p. 56, note) refers, though not without hesitation, to a calculation by G. Bishof that "the elastic force of steam is at its maximum when it has the same density as water." This appears to imply that at temperatures above the critical it becomes more compressible. "Natural System of Volcanic Rocks," *California Academy of Sciences*, Vol. I., Part II. San Francisco, 1868.

To express the increment of pressure due to the increment dx in the depth we must have

$$dp = g\mu dx$$
$$= g\beta \frac{p}{Vp + \alpha} dx.$$

Integrating

$$Vp + \alpha \log p = g\beta x + C.$$

At the bottom of the crust the depth is k and the pressure $g\rho k$;

$$\therefore\ V(g\rho k - p) + \alpha \log \frac{g\rho k}{p} = g\beta (k - x),$$

which is the equation connecting the pressure with the depth.

To determine the relation among the constants we have already seen that

$$\mu = \frac{\beta}{V + \frac{\alpha}{p}}.$$

At the bottom of the crust the density is σ, which we have hitherto assumed to be that of basalt, and the pressure is there $g\rho k$, whence

$$\sigma = \frac{\beta}{V + \frac{\alpha}{g\rho k}}.$$

If then the mass of lava under consideration be taken as that which occupies unit of volume when it is in the state of magma, we have

$$V + \frac{\alpha}{g\rho k} = 1.$$

And $\qquad\qquad \therefore\ \sigma = \beta.$

That is to say that the mass of unit of volume of the magma is equal the mass of unit of volume of basalt. Consequently if the rock constituent be of the density of basalt the water constituent must be so likewise, which agrees with our previous conclusion.

Substituting for α and β, our equation may now be written

$$\frac{1-V}{V}\log\frac{g\rho k}{p}-\frac{p}{g\rho k}=\frac{1}{V}\frac{\sigma}{\rho}\frac{k-x}{k}-1.$$

We are so little acquainted with the behaviour of matter under the circumstances which environ it beneath the earth's crust that it is dangerous to speculate upon the laws which govern it, but it is thought that the consequences of the hypotheses above made will be sufficiently near the truth to justify our arguing upon them.

At the surface $x=0$, and then

$$\frac{1-V}{V}\log\frac{g\rho k}{p}-\frac{p}{g\rho k}=\frac{\sigma}{V\rho}-1;$$

whence

$$V=\frac{\log\frac{g\rho k}{p}-\frac{\sigma}{\rho}}{\log\frac{g\rho k}{p}-\left(1-\frac{p}{g\rho k}\right)}.$$

The condition that $V<1$ is satisfied if

$$1-\frac{p}{g\rho k}<\frac{\sigma}{\rho}.$$

Now $\frac{p}{g\rho k}$ is necessarily <1, and $\frac{\sigma}{\rho}>1$. Consequently this condition is always satisfied, and therefore if there be any water-substance at all in the magma it will raise the lava to the surface under the supposition that the same identical water remains always in the same portion of lava.

If $V=1$ we obtain $p=-(\sigma-\rho)k$. This value is without meaning, but being the same that we obtain from the equation

$$p=g\sigma\int dx+C,$$

after determining the constant by the condition that $p=g\rho k$ at the bottom of the crust and then giving to x the value zero, that being the value we should arrive at when there is no water in the lava, it verifies our result.

In the equation which connects the pressure p of the lava at the surface of the crust with V the proportion of rock substance in the magma, if we assume one of the quantities we can find the other, but we have no sure guide to lead us to the choice of either of them. We will assume $V = 0{\cdot}9$, and it will hereafter appear when we are treating more particularly of volcanic action that this assumption is not unreasonable. If we put $\frac{g\rho k}{p} = n$ in the equation for the pressure at the surface and assume the usual values for ρ and σ, then with the above value for V it may be put into the form

$$n\,(\log n - 2{\cdot}044) = 9.$$

Using a table of hyperbolic logarithms we find by trial that the value 14·4 substituted for n gives 8·97 and the value 14·5 gives 9·13. Hence 14·4 is a near approximation, and the pressure of the lava at the level of the surface of the crust would be that due to a column of the crust $\frac{25}{14{\cdot}4}$ or 1·7 miles high. The weight of a column of vesicular lava would not be so great as that of a column of solid crust of equal height. We see then that if $\frac{1}{10}$th of the volume of the magma consisted of water-substance it would be sufficient to produce an overflow of lava from a lofty crater, or to raise a range of mountains consisting of eruptive rocks above an elongated fissure.

The relation

$$dp = g\beta \frac{p}{Vp + \alpha}\, dx$$

enables us to find the whole horizontal pressure upon the sides of a fissure which is filled with lava under the conditions implied by it.

For
$$\frac{1}{g\beta}(Vp + \alpha)\, dp = p\,dx\,;$$

$$\therefore \quad \frac{1}{g\beta}\left(\frac{Vp^2}{2} + \alpha p\right) = \int p\,dx.$$

But $\int p\,dx$ taken from the surface to the bottom of the crust is the whole pressure upon a vertical section of the side of

the fissure. If we substitute the values already found for β and α in this equation, it becomes

$$\frac{1}{g\sigma}\left\{\frac{Vp^2}{2}+g\rho k(1-V)p\right\}=\int pdx.$$

Now at the bottom of the crust $p=g\rho k$, and putting $V=0{\cdot}9$ we have found that the value of p at the surface will be $\frac{g\rho k}{14{\cdot}4}$. And $\frac{\rho}{\sigma}=0{\cdot}905$.

Using these values at the limits we obtain

$$g\rho k^2\times 0{\cdot}489=\int_0^k pdx.$$

And if we give to k the value 25 miles

$$g\rho\times 12{\cdot}2 \text{ miles}=\frac{1}{k}\int_0^k pdx.$$

That is to say the mean horizontal pressure of the lava throughout the vertical section is equal to what would be produced by the weight of a column of the crust rather more than 12 miles high.

The work done by the compressing force in raising a mountain chain (which we use as a generic term for any elevated tract) will consist of two parts, (1) the work producing deformation of the crust, which will include the work of distortion and cleavage, and is done against the molecular forces, and (2) the work done against gravity. This last will in itself consist of two parts, viz. the elevation of the mass of the mountain above the upper mean level of the crust, and the depression of its root into the fluid, below the lower mean level.

If we take $y=f(l)$ to define the contour of the elevated mass along a section across the tract, the work against gravity on a section of unit of width will be

$$g\rho\int\frac{y^2}{2}dl.$$

And similarly, if y' be the ordinate corresponding to y for the root of the mountain below the lower mean level, the

work against gravity in depressing the root will be

$$g(\sigma-\rho)\int\frac{y'^2}{2}\,dl.$$

But
$$y'=\frac{\rho}{\sigma-\rho}y.$$

Hence the whole work against gravity, which is the sum of these, will be

$$g\left\{\rho+(\sigma-\rho)\frac{\rho^2}{(\sigma-\rho)^2}\right\}\int\frac{y^2}{2}\,dl.$$

Now if M be the volume of the mountain, and m the height of its centre of gravity above the upper mean level,

$$\int\frac{y^2}{2}\,dl=mM\,;$$

and the work done against gravity will therefore be

$$g\rho\left(1+\frac{\rho}{\sigma-\rho}\right)mM.$$

Again, if A be the area of the fissure which is exposed to the pressure t, and le the space through which it is opened by the action of t, then the work done by t is $tAle$.

But since the opening of the fissure gives rise to the elevation above and depression below the mean crust, it follows by equality of volumes that

$$Ale=M+\frac{\rho}{\sigma-\rho}M.$$

Hence the work of compression is equal to

$$t\left(M+\frac{\rho}{\sigma-\rho}M\right).$$

Consequently,

$$\frac{\text{work done against gravity}}{\text{work of compression}}=\frac{g\rho m}{t}.$$

If the fissure be closed above, this $=\dfrac{\rho m}{\rho k-\sigma h}$,[1]

[1] p. 187.

where h is the height to which the pressure t allows the magma to rise in the fissure. In the most favourable case $h = 0$, and then,

$$\text{work done against gravity} = \frac{m}{k} \text{ work of compression.}$$

It is of course to be understood that what we have spoken of as the fissure of area A and width le is in reality the aggregate of all the fissures which occur within the length l of the tract, each of which is under an equal, or nearly equal, tension t, so that the compressing action takes effect throughout that length.

The height m of the centre of gravity of the elevated tract is evidently very small compared with the thickness of the crust k, for the entire mean altitude of the continents is put at only 1000 feet, so that $\frac{m}{k}$ is probably considerably less than $\frac{1}{100}$ in an extreme case. We see then that but a very small part of the available force, arising under the conditions suggested, will be expended in raising the mountains, and by far the larger part of it will be available for overcoming the molecular forces which resist distortion of the rocks.

But if a fissure be open to the surface so that a massive eruption takes place through it, then the pressure t, statically considered, upon the side of the fissure is what we have found in that case to be about $g\rho \times 12{\cdot}2$ miles, which is less than the pressure that would arise, if it were filled with a liquid of the density of the crust. This latter obviously could not produce compression. Hence, after the lava has gained access to the surface, the lateral force may cease to be effective, which agrees with the common observation, that earthquakes cease when a volcano breaks into action. In this limited sense the saying is justified, that volcanoes are the safety valves of the globe.

Now it is not probable that any considerable volume of the crust could support continuously a horizontal pressure much exceeding the vertical pressure without becoming distorted. If these orthogonal pressures were equal, they would tend only to

compact the rock, and render it more dense. But if the horizontal were considerably to exceed the vertical, the rock would be sheared upwards and downwards. Whether the force at our disposal under the hypothesis now offered would be sufficient for this purpose is of course open to argument, but considering that, with time given, all known substances yield more or less to deforming forces, it seems quite possible that it may be so. And the facts which we shall bring forward presently make it almost certain that the work has been effected in this manner[1]. Moreover the rocks fissuring from contraction, will detract from their rigidity, and render shearing more easy, when large volumes of them are under consideration.

If our chasm did not extend upwards to the surface, when the tension of the vapour in it diminished through cooling the lava would ascend further, until it became too viscous to rise higher. At such a juncture we might have vapours still at a very considerable pressure occupying the upper portion of the fissure, while the lower part would be filled with the magma, solidified at its upper surface and more and more fluid below. In this manner we may conceive gaping chasms to have been formed, which might be for a long time occupied by heated vapours, and gradually converted into mineral veins.

When the molten rock injected by hydrostatic pressure into the fissures came to solidify into the dykes which so abundantly intersect metamorphic strata, it is probable that another efficient cause of compression would come into play, for, as already mentioned, the result of recent experiments appears to show that such substances as whinstone and granite are less dense in the solid than in the liquid state at the melting temperature[2]. In this respect then they behave like water.

[1] pp. 203, 204.

[2] p. 23. If this fact be established, then it appears that pressure lowers the melting temperature of such rock masses. But it has been considered proved that the greater portion of the globe is extremely rigid, and it has been considered to owe its rigidity to pressure. We must conclude then, either that the central part consists of materials of a different kind from those which have yielded these experimental results, or else that it is cool. The former supposition is the more likely to be true.

Indeed there are points of analogy so close between the circumstances of ice resting upon water, and such a crust as we have supposed resting upon a fluid substratum, that we may expect to receive suggestions for explaining the phenomena of crust movements from the behaviour of ice. In both cases the floating substance is but slightly less dense than that upon which it floats. In both cases the one is incorporated with the other by congelation. In both cases the floating substance has a considerable amount of rigidity, but yields somewhat freely to differences of pressure when sufficient time is allowed. These analogies have been noticed by many. Thus, in travelling across the frozen sea to the north of Spitzbergen, Prof. Nordenskiöld gives this description of the condition of the ice and deduces conclusions from it regarding movements in the crust of the earth:

"The ice we passed is formed not of colossal blocks or icebergs, but of angular blocks of ice, *not waterworn,* piled loosely over each other, so as to form pyramids, or walls of ice, up to thirty feet high, which were so close to each other that the space between them was frequently not large enough for our tent.

"The cause of the formation of these ice-walls, which were also observed by Wrangel on the north coast of Siberia, is probably to be sought for in the changes of volume which ice undergoes when its temperature is changed. According to Plücker and Geissler the linear expansion-coefficient of ice is $=0{\cdot}0000528$. If, therefore, ice at 0° C. be cooled to -15° C., cracks must arise which, for 1,000 metres, have a breadth of thirty-two inches. The cracks naturally freeze together immediately afterwards," [which must mean that they become filled with water and the water in them freezes], "and when the ice is again warmed, for instance to -5°C., a piling-up must take place of twenty-one inches per kilometre. During the course of the winter this phenomenon is repeated innumerable times, one layer of ice being piled upon another, till the whole ice-field forms a confused mass of blocks heaped up against each other. Similar forces are also in operation in the crust of the earth, with less intensity, indeed, in consequence of the smaller

expansion-coefficient of the rocks which compose it, and the inconsiderableness of the changes of temperature which occur in them, and the cracks thus formed may here come together again, provided no chemical or mechanical sediment has been deposited in them, as is, perhaps, often the case. On the other hand, the forces operate in the earth's crust during millions of years, and I doubt not that in the circumstances here noticed the cause of the strata being contorted, dislocated *and thrown over each other*, is to be sought for. This last, perhaps, to judge by the observations I had the opportunity of making on the polar ice, happens far oftener than we commonly suppose, and when it takes place there often occurs no considerable disturbance in the original horizontal position of the stratum. Certainly in most cases the veins filled with foreign minerals, by which the upper strata of the earth in particular are intersected in all directions, derive their origin from similar causes; that is to say, from cracks which have, in consequence of changes of temperature, many times over opened and come together again, *provided they were not prevented by the falling in of débris.* This has however often taken place, considerable masses of sediments, formed chemically or mechanically, have frequently collected in the cracks, and during the immense duration of geological ages they have hardened and been metamorphosed to solid crystalline rocks, limestone, quartz, felsite, pegmatite, &c."[1]

We find in this extract a theory of the compression of the strata of the earth, proposed by a philosopher of high repute, and no doubt suggested to him by the analogy between ice and rock. It is remarkable however that he does not attribute any part of the compression of ice to the expansion of the water when it freezes in the cracks, and he has missed that part of the analogy on which we insist, which arises from the flotation both of the ice and of the earth's crust. It is rather to a cause analogous to the expansion of the freezing water, than to variations of temperature in the ice, that we should be disposed to attribute such part of the compression as may possibly be connected with the solidification of fluid rock in chasms. It will be noticed

[1] "The Arctic Voyages of Adolf Erik Nordenskiöld," 1858—1879. London, Macmillan, 1879, p. 239.

that Prof. Nordenskiöld does not make any reference to such injection of fluid rock into the earth's crust.

The ice upon a sheet of water that has been much skated on, after a few days' frost assumes an appearance very much akin to that of a moderately disturbed area of the earth's crust. This no doubt arises from the cracks which are formed, and by day are filled with water, which during the night freezes and expands, ridging up the ice on both sides of them. The transference of sediment from one locality to another has an effect analogous to the irregular distribution of load by the skaters' movements, and produces cracks opening upwards from below, carrying out the analogy.

But besides the unequal pressure caused by local deposits of sediment, which must tend to crack the crust from below upwards, the mere contraction of sedimentary rocks, when they sink by becoming overloaded into hotter depths, and there become metamorphosed and assume a denser character, must cause them to crack during the process. It is to the opening of cracks *from below* that we now venture to attribute the sequence of operations which result in the corrugation of tracts of the earth's surface. It necessarily follows that this action is the consequence of changing conditions, and is to be looked for where sediment is being deposited, and sedimentary rocks are becoming metamorphosed as along the shores of continents, rather than beneath the quiescent depths of the profound oceans, where the conditions are more fixed and changeless. Thus we are again led to regard compression as a continental phenomenon.

Prof. James Hall, Dr Sterry Hunt, Prof. Joseph LeConte and other American geologists connect the exhibition of compression of the crust with those regions of the earth's surface where thick sedimentary deposits have been previously laid down. Thus they conceive the corrugation of the Appallachian chain to have been a consequence of the extremely thick series of deposits of nearly the same age, of which that range consists. In the lecture already quoted[1], Dr Sterry Hunt thus makes reference to a former publication by himself[2]: "The effects of heat and

[1] p. 94.

[2] "American Journal of Science and Art," May, 1861 (II., xxxi. 411).

water upon the buried sediments would be *condensation*, from the diminution of porosity and still more from the conversion of the earthy materials into crystalline species of higher specific gravity, thus causing *contraction* of the mass. A further and very important result of this accumulation there pointed out was by the softening of the underlying floor, or the '*bottom strata to establish lines of weakness or of least resistance in the earth's crust, and thus determine the contraction which results from the cooling of the globe to exhibit itself in those regions, and along those lines where the ocean's bed is subsiding beneath the accumulated sediments.*' Hence, I added, 'we conceive the subsidence invoked by Mr Hall, though not the sole nor even the principal cause of the corrugations of the earth's strata, is the one which determines their position and direction by making the effects produced by the contraction, not only of sediments but of the earth's nucleus itself, to be exerted along the lines of greatest accumulation'"[1].

The theory so epitomised appears to have gained very general acceptance among physical geologists. But if the reasoning of the earlier chapters of the present volume is correct, the cooling of the globe cannot be appealed to to furnish the requisite compression (in the above extract called "*contraction*"), much less would it furnish a sufficient balance of compression along a belt of crust, itself contracting through metamorphic action. But upon the view now suggested, the circumstances fit in very well. The thick deposit depressing the crust, induces metamorphism in its lower portion. This cracks in consequence. The magma fills those cracks with highly expansive vapour, and compression is the result.

It is also important to remark, that the vapour pressure in a chasm would be increased when the crust was thickened, because the tension at the bottom of the crust is proportional to its thickness. On this account, although it would require a greater horizontal force to compress the crust where it is thicker, yet the requisite increase would be furnished to the force by the very fact of the increased thickness. If our theory of the

[1] "American Journal of Science and Art," 1873, Art. XXVIII. p. 265.

formation of volcanic vents be accepted, it will be also apparent that the force which opens them will for the above reason be proportionally greater where the crust is thicker, so that their occasional occurrence on lofty plateaux is no argument against the theory.

The conclusions to which we have been led as to the cause of compression, and of the elevation of mountain ranges, have been arrived at independently, and entirely upon the grounds already mentioned. They appear however to be decidedly in accordance with geological observations, and agree well with the history given by Prof. Judd, in his recent work on *Volcanoes*, of the connection between volcanic activity and the elevation of mountain ranges[1]. He there shows that the Alpine chains have been raised contemporaneously with the manifestation of strong volcanic outbursts along lines parallel to them. He appears to adopt in general what we may call the American theory of the formation of mountains, but he supplements it by specially connecting volcanic outbursts with the actual throes of their elevation. He does not appear however to trace the connection of cause and effect between the two as we have done. "The movements which resulted in the crushing and crumpling of the thickened mass of sediments along the Alpine line of weakness, also gave rise to the formation of a series of fissures from which volcanic action took place[2]." We should however expect that, had the crushing and crumpling arisen from any external cause, it would rather have kept fissures closed than opened them. It is the synchronism to which we desire to draw attention. Thus again: "We have abundant evidence that just at the period when these great movements were commencing which resulted in the formation of the great Alpine and Himalayan geanticlinal, earth-fissures were being opened upon either side of the latter[3] from which volcanic outbursts took place. At the period when the most

[1] "Volcanoes," Chap. x.

[2] *Ibid.* p. 297.

[3] The word "latter" strictly should confine this assertion to the Himalayan range. But from the enumeration of the localities, most of which are European, this cannot be intended. *Ibid.* p. 298.

violent mountain-forming movements occurred, these fissures were in their most active condition, and at this time two great volcanic belts stretched east and west, on either side of, and parallel to, the great Alpine chain."

Of the same purport are the remarks of Baron Richthofen on the connection between the distribution of volcanic rocks and elevated regions of the surface. He asks, "Was the particular structure of certain portions of the crust of the globe, which is indicated by the situation of elevated regions on its surface, among the causes of the eruption of volcanic rocks? or were the inequalities of level on the surface, in the volcanic regions, due to the processes attending and the agencies causing the eruptions? The answer to both these questions must be in the affirmative[1]."

The same author writing of the mode of geological occurrence of the rocks which have formed massive or fissure eruptions, says of andesite that "preeminently the greater part of it has to all appearance been ejected through extensive fissures; that is, it has been produced by what are styled massive eruptions...... Andesitic mountains are characterised by monotony in scenery. They form continuous ranges which are often of considerable elevation and extent[2]." Again of this rock he tells us that in Hungary, "andesite composes entirely the Hargitta-range, which extends over one hundred miles in length and twenty-five in width; the Vihorlat-Gutin-range, which is of still larger dimensions, and the Eperies-Kaschau-range; all of which are densely wooded, and of a gloomy monotonous aspect[3]." Similarly he mentions belts and ranges of basalt[4].

If the eruption of these extensive ranges, through fissures which must have been of great width, was indeed synchronous with the compression that raised the mountains which they skirt, there appears to be no escape from the conclusion that the rending of the fissures was the cause of the compression, because no pressure from beyond, greater than their own, could have

[1] "Natural System of Volcanic Rocks," p. 82.
[2] *Ibid.* p. 75. [3] *Ibid.* p. 30.
[4] *Ibid.* p. 27.

been transmitted across such fissures filled with fluid, nor could they have supported a pressure propagated from the further side of the elevated tract, so that the intervening region could have been compressed.

According to the suggestions offered in the present chapter to explain the compression of the crust, the store of energy from which the mechanical force is derived resides (1) in the heat of the interior, and (2), if we refer any part of it to expansion of the rocks on solidifying, in the molecular force attending a change of state; while in the theories discussed in the former chapters the force is derived from the gravitation of the crust towards the centre of attraction of the globe. This latter force is practically infinite compared with the work it would have to perform in shearing the crust, but it has been shown that the limit to the space through which it can have acted, owing to the small amount of the globe's contraction, is so quickly reached, that it cannot fully explain the phenomena. The case is entirely different with regard to the theory now suggested. Here the force (1) is limited, since it never can exceed, or indeed practically reach, the pressure of a column of the earth's crust, viz. about 23,547,900 pounds, or 10,066 tons, upon the square foot, but there is no limit to the space through which it might act, so long as the fissures did not reach the surface. The limit imposed is the resistance which the crust offers to shearing. And it is probable that this may have been reached, where a tract has been thickened to such an extent, that its surface has been elevated to the normal height of the highlands of the globe.

The direction orthogonal to a fissure in which motion would take place when the crust yielded, would depend upon its rigidity; and it is conceivable that, although thicker beneath continental areas, it may be weaker there, because less homogeneous and less dense; and we know that surfaces of rupture, once formed and faced with slickenslide, are ready prepared for repeated movements. If such be the case, the tendency would be for compression to take place in the direction from the shore margin towards the mountain ranges. The latter being continu-

ally denuded down, and the material carried to the shore margin again, and there accumulating, sinking, and being again pressed towards the ranges, would cause a kind of circulation of rock from the coast-line inland, and back again by way of the surface. Since by this means every part would in its turn be subjected to atmospheric action, the more soluble basic constituents would be carried out to sea, and it is possible that it may be to this cause that the lesser density of the continental crust may be owing, even although the continental rocks have these constituents to some extent continually restored by the intrusions of the magma from below[1]. It has been ascertained that in the deeper parts of the ocean, from 2700 fathoms and more, the dredge invariably brings up a fine red clay, consisting of silica, alumina, and oxide of iron; and everywhere containing nodules of the peroxide of manganese[2]. It matters not what the mode of transference and deposit of this clay may be, whether directly as sediment, or indirectly, as believed to be the case, from the insoluble remains of animal exuviæ, for what is material to our present argument is, that these heavy minerals, iron and manganese, are accumulating over the floor of all the deeper parts of the ocean, thus rendering the mean density of the crust increasingly greater there, and necessarily abstracting these substances from elsewhere, which can be nowhere except from the continental areas, and diminishing their density by that means.

brought down of canic pumice

The relation between the amount of compression, and the quantity of lava injected into and solidified in the fissures, is easily found. Referring to the figures on page 46, suppose the tract, which before compression gave the rectangular section *ABCD*, to be distorted by the opening of fissures. These will become partially or wholly filled with lava. The parts of them which do not become so filled, become filled with debris, forming veinstone, and this will be derived from

[1] See an interesting paper on the stratified rocks considered with respect to the contributions which the internal parts of the globe have made towards them by M. Daubrée, "Bulletin de la Société de France," 2ce Série t. XXVIII. p. 363. Séance du 7 Sep. 1871.

[2] "Nature," Vol. XI. p. 116. Dec. 10, 1874.

the pre-existing crust. But the solidified lava is a new accession to it.

If $\Sigma(f)$ be the volume of the lava injected into the fissures, we shall have by the figure,

$$kl + \Sigma(f) = kl + \Sigma(a) - \Sigma(b) + \Sigma(\beta) - \Sigma(\alpha),$$

or

$$\Sigma(f) = \Sigma(a) - \Sigma(b) + \Sigma(\beta) - \Sigma(\alpha).$$

If e were the compression which would produce a similar tract out of the length l and thickness k of crust we perceive from this expression that

$$\Sigma(f) = kle.$$

We have estimated the value of e to be in the case of nature[1] about 0·02; taking the thickness of the crust as 25 miles. With these values we get

$$\Sigma(f) = \tfrac{1}{2}l;$$

that is to say the vertical sectional area of the dykes, occurring in each horizontal mile of crust 25 miles thick, would be half a square mile, or one-fiftieth of the whole sectional area. This does not appear unreasonable, especially when we remember that the dykes will be most numerous and widest in the lower parts of the crust, which are probably never brought under our observation.

[1] p. 179.

CHAPTER XVI.

ON FAULTING.

Faulting due to contraction—"Hade to the downthrow"—Faults may die out in the depths—The fissure which gives rise to the fault—Fissure will have the same hade as the fault—Course of fault in a straight line—Fissures which cause faults commence at the surface—Faults of Utah described by Captain Dutton—Faulting superimposed on corrugation—Extent of throw of great faults—Faults will not usually admit the fluid magma—If they do so may give rise to fissure eruptions of igneous rock—Application of datum-level equation to faulting—Faulting must depress the tract affected—General theoretical conclusion respecting the connection of faulting and corrugation.

BESIDES corrugation the strata of the earth's surface have been extensively affected by faulting. This has been usually, and probably correctly, attributed to the contraction and consequent shortening of the crust over those areas where it occurs; for by faulting a portion of the crust is enabled to occupy the same horizontal area after contracting, that it did before, without gaping fissures being formed in it.

The well-known fact that faults commonly "hade to the downthrow," which in untechnical language means that the divisional plane, which constitutes the fault, inclines downwards towards the side upon which the corresponding beds are found at a lower level, appears to be explicable in the following manner. If we had a series of superimposed beds perfectly homogeneous each in itself, and each having its upper and under side strictly parallel and horizontal, and if contraction were to commence in these at the upper surface, it is conceivable that vertical cracks might be formed in them, dividing the whole area into

blocks, as we see in the drying mud of a pond that has been drained. Afterwards, or rather concomitantly, the vertical pressure arising from the weight of the material in the blocks might cause the cracks to be again filled up, at any rate towards the bottom of them, by pressing out the material of the blocks laterally so as to fill up the cracks. In such a case there would be neither throw nor hade, and in fact no fault at all, but merely a divisional plane or joint.

But if after a V-shaped fissure had been formed, running say North and South, the strata on one side, say the East, were to settle downwards more than those on the West, the material upon the East side would then be pressed out laterally, by this settling, against the West side of the fissure. This side of the fissure would accordingly form the divisional plane of the fault, and would necessarily slope downwards to the East, towards the sunken strata, or "hade to the downthrow." For the sake of simplicity the two movements of the opening of the fissure, and the settling down of the strata, have been supposed to have occurred separately. But in nature the two movements would go on together, and the pressure of the subsiding beds would support those on the opposite side, assisting to prevent their settling down to the same extent as if they were not so supported.

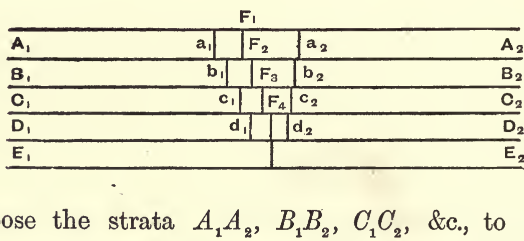
If this explanation be admitted, it appears that an ordinary fault of small throw might be formed through a very slight defect from homogeneity of the beds on either side of it, for the open space afforded by the fissure itself would give a certain amount of room for the downcast beds to expand into when they subsided. But if there was a considerable change in passing from West to East in the condition of the beds, the less dense layers becoming thicker and the harder ones thinner within a moderate distance of the fault on the downcast side, then a larger displacement would result.

It is obvious that this explanation of the phenomena would require the throw of the fault as a rule to become less and less at greater depths, as is believed to be the case. Indeed the Author has a note of having seen, in stratified brick-

earth in a pit at Copford in Essex, the complete phenomena of an ordinary fault exemplified in miniature within the thickness of about a foot, the dislocation being greatest at the upper part, where the layers had been denuded to a level and again covered up unconformably, and dying out entirely at the bottom of the fault. In ordinary strata sections cannot be seen of sufficient depth to exemplify this fact; but Mr Curry informs us[1] that "it is found, especially in the Alston Moor district, that the throws and shifts are greatest at and near the surface, and that they diminish in descending into the earth." He also mentions the fact that "hades are greatest in shales, or argillaceous strata," a fact agreeing well with the explanation above offered of their origin.

When a V-shaped fissure is propagated downwards the angle of the V will follow some definite course or path in its descent through the strata. We may call this the axial surface, or, with sufficient accuracy, the axial plane of the fault. It is obvious that, according to the above theory, it will not coincide with the plane of the fault itself, which may be coincident or nearly so with one of the branches of the V. Even should the axial plane be vertical, the fault need not be so; as has been explained above.

But it seems more probable that the axial plane will have an inclination corresponding to the hade of the fault, though not so great. Of this the following *quasi* proof is offered.



Suppose the strata A_1A_2, B_1B_2, C_1C_2, &c., to be superimposed; and suppose that a fissure is commenced by A_1A_2 cracking at F_1. Conceive the strata to contract more towards

[1] "Proc. Literary and Phil. Soc. of Manchester," Vol. IX. p. 44. 1869.

A_2, so that when contraction has proceeded to a small extent, the end a_2 of the ruptured stratum has moved further from its original position F_1 than a_1 has moved from the same place. Now let the stratum B_1B_2 crack. It is probable that it will crack at F_2, half way between a_1 and a_2. At any rate, the probability that it will crack to the right being the same as that it will crack to the left, for a large number of layers the average position of the crack will be central. Now, if the thickness of the strata be indefinitely diminished and their number indefinitely increased, we get the actual circumstances of the formation of the fissure, and the locus of the points F_1, F_2, &c. is the axial surface; and it is obvious that if our demonstration be admitted it will hade to the downthrow, because the beds which contract most in the horizontal direction will also contract most in the vertical and form the downcast side of the fault.

The fault itself would also have its throw and its hade in the same direction as the fissure, and would run on slantingly downwards until it reached some stratum or massive rock which did not contract at that place, or where unconformity occurred and contraction had previously been satisfied in the underlying beds.

It would seem that vertical rather than horizontal contraction (though the two would go on together) determines the course of faults. For horizontal contraction would tend to divide a country into polygonal areas like a bed of basaltic columns, but faults run in approximately straight lines. Now this is the direction followed by a crack which is formed by the depression of a lamina as may be observed in the case of ice. When one first steps on a sheet of ice that has never been disturbed it is usual to notice a crack run with a ringing sound, quite, or nearly, across the surface in a straight line from beneath one's feet. This is of the nature of a fault and is another instance of the analogy between the behaviour of ice and the strata of the earth's crust.

We propose therefore to explain the phenomena of faulting

by the occurrence of fissures, commencing at the surface and running downwards; and among the causes which may have given the first impetus to their formation may possibly have been the drying of a surface from which either the sea or inland lakes had been drained off, or even where the climate had changed and become arid. That some faults have originated on land surfaces appears from the observations of the United States Survey, who tell us that in the High Plateaux of Utah "The great displacements began in early tertiary time, and are probably yet in progress. The evidences of the recency of some of these movements appear in the escarpments frequently seen along the line of faults where Quaternary beds have been broken at a time so recent that the escarpments have not been destroyed by atmospheric agencies, and further evidence is exhibited in the small amount of talus frequently found at the foot of a recently formed fault-scarp[1]."

The connection of faults with surface changes is indicated by the following passage from Captain Dutton's work: "It yet remains to speak of another interesting relation of the later system of faults. They have throughout preserved a remarkable and persistent parallelism to the old shore line of the Eocene lake following the broader features of its trend in a striking manner. The cause of this relation is to me quite inexplicable, so much so, that I am utterly at a loss to think of any subsidiary facts which may be mentioned in connection with it and which can throw light upon it[2]." It appears that the area of the Eocene lake has been raised by the faulting[3]. May not the removal of the superincumbent weight of water have been the proximate cause of the disturbance, and of the uprising of the lightened area?

Indeed we might expect that all such readjustments of the equilibrium of the crust as do not arise immediately from

[1] "Report on the Geology of the High Plateaus of Utah, by C. E. Dutton, Captain of Ordnance, U.S.A. Prefatory note by Major Powell," p. viii. Washington, 1880. A description of perhaps the most magnificent system of faults in the world is to be found in Chap. II. of this work.

[2] *Ibid.* p. 45. [3] *Ibid.* p. 38.

horizontal compression would be effected by faulting. The features produced by this kind of movement would therefore be superimposed upon the features previously or subsequently determined by compression and consequent corrugation.

The blocks into which the strata were cut up would on the whole follow the "lie" of the country as previously or concomitantly determined by the forces of compression, a general law thus lucidly enounced by Prof. Green: "We find that we can account for the observed facts only on one supposition, and that is, that *the rocks have been folded into a series of troughs and arches, or thrown into domes and basins.* This is the great general law which governs everywhere the arrangement of the disturbed portions of the earth's crust. Faulting, or violent contortion and inversion, often complicate and obscure this structure and interfere with its symmetry, but never to such an extent as to prevent its being recognised as the great leading feature in the arrangement of the rocks[1]."

Faults occur of very different extent of throw, varying from a few inches to many fathoms, or even miles. In Captain Dutton's "Geology of the High Plateaus of Utah" we read of displacements by faulting of 5,500, 1,800, 12,000, 4,000, and 7,000 feet[2]. Prof. Bonney tells us that the perhaps largest known fault in the world is in the Appalachian Chain, where, on opposite sides of a crack over which a man can stride, beds are brought together that were once 20,000 feet apart[3].

It is of course possible that a fault may cut quite through the earth's crust, and indeed where the throw is so great as to be measured by several thousand feet, as in some of the larger faults referred to, it is scarcely conceivable that it should not do so, if our estimate of about 25 miles for the thickness of the crust is correct. If however the fault originate in a fissure formed by the contraction of the strata near the surface, and they settle together as the fissure extends downwards, thus forming wedge-shaped blocks, broader at the top than the

[1] "Geology for Students and General Readers," 1876, p. 374.

[2] Chap. II.

[3] "Manuals of Elementary Science. Geology." S.P.C.K. 1874, p. 43.

bottom, on account of the direction of the hade, it seems that the fissure will be kept closed, and not admit the magma from below. If however the contraction were very nearly equal on both sides of the fissure, so that there was no greater settlement on one side than on the other, a fissure might be actually opened downwards through the whole thickness of the crust; and being open above so as not to form a chamber for the accumulation of high-pressure vapour to prevent it, the magma would rise into it, and one of those great fissure eruptions would ensue, which have, at one or another geological epoch, covered large portions of the globe with beds of igneous rock. The notion of a vast crevasse extending downwards to the fluid magma may seem a wild dream, and certainly has no parallel in anything known to occur at the present day. But neither has any event of the nature of a fissure eruption come under the cognizance of man; and yet their former occurrence seems incontestible. And consequently the existence of such fissures becomes a necessity[1].

The datum-level equation, (1) p. 47, is at once made applicable to the case of faulting, when that arises from contraction of the rocks, by making e negative. But since the rocks are supposed to contract in the vertical as well as in the horizontal direction, e will represent the quadratic contraction of the vertical section; that is to say, upon the contraction taking place, the sectional area kl will become the area $kl(1-e)$.

We shall then have,

$$kle = \Sigma(b) - \Sigma(a) + \Sigma(\alpha) - \Sigma(\beta).$$

It is easily seen that this is correct, because the area of vertical section, which was before contraction occupied by kl, is after contraction occupied by

$$kl(1-e) + \Sigma(b) - \Sigma(a) + \Sigma(\alpha) - \Sigma(\beta);$$

[1] "The Lava Fields of North Western Europe," Prof. A. Geikie. *Nature*, Vol. XXIII. p. 3. 1880. Also Richthofen's "Natural System of Volcanic rocks," p. 17. *Mems. of California Academy of Sciences*, Vol. I. Pt. ii. San Francisco, 1868.

whence, equating these quantities,

$$kle = \Sigma_i (b) - \Sigma_i (a) + \Sigma_i (a) - \Sigma_i (\beta),$$

which is the datum-level equation with the sign of e changed.

It follows as before[1] that

$$\frac{\Sigma_i (b) - \Sigma_i (a)}{\Sigma_i (a) - \Sigma_i (\beta)} = \frac{\sigma - \rho}{\rho},$$
$$= 0{\cdot}104,$$

if we suppose σ and ρ to be the densities of basalt and granite. Combining this with the expression for kle we get,

$$\Sigma_i (b) - \Sigma_i (a) = 0{\cdot}09\, kle.$$

Whence $\Sigma_i (b) - \Sigma_i (a)$ is a positive quantity.

Indeed generally,

$$\Sigma_i (b) - \Sigma_i (a) = \frac{\sigma - \rho}{\sigma} kle;$$

and is positive because σ is greater than ρ.

But $\Sigma_i (b) - \Sigma_i (a)$ is the mean depression of the tract below the mean level. Hence we see that such faulting as arises from subsidence owing to contraction, must on the whole depress the surface of the tract disturbed by it. But the depression will be small because $\dfrac{\sigma - \rho}{\sigma}$ is small on account of the difference of the densities being small compared with the greater.

The general conclusion now offered for the consideration of geologists therefore is, that both corrugation and faulting are ultimately dependent upon the contraction, and consequent fissuring, of the crust, when it undergoes those changes, varying in degree from mere solidification to intense plutonic action, which convert aqueous sediments into denser rocks, and are all included under the generic term, metamorphism. The fissuring consequent on the contraction, which accompanies these changes,

[1] p. 118.

when it commences below and is propagated upwards, gives rise to the phenomena of compression, owing to the intrusion of highly elastic matter from below, and corrugation is the consequence. Volcanic phenomena are believed to be another manifestation of the same mode of action. On the other hand, when the fissures commence above and are propagated downwards, they give rise to ordinary faulting, and if formed on so large a scale that they reach through the crust, it is suggested that great fissure eruptions of igneous rocks may take place through these.

CHAPTER XVII.

GEOLOGICAL MOVEMENTS EXPLAINED.

Recapitulation of the four suggested causes of compression—Three of them negatived—The fourth possibly the true one—The fact of compression certain, and the duplex character of the corrugations most probable—The consequences of denudation followed out—Elevation its correlative—Fresh-water strata covered by marine—Movements most energetic near, but not confined to, continents—Degradation of mountains a law of nature—Enormous thickness of certain strata accounted for—Raised sea beaches—Two classes of elevatory movements—Drainage across dip—Theories of compression compared.

In our endeavour to discover the cause of the compression, by which it is generally believed that the elevations existing upon the earth's surface have been produced, it must be admitted that we have not arrived at any degree of positive certainty. The results obtained have been mostly negative. But even negative results have great value, because the disproof of a theory, which for want of close examination may long hold its ground, removes a chief obstacle to further advance in knowledge. It is submitted, then, that the theory has been disproved (1), That the earth is a solid body cooling by conduction, and that the inequalities which appear on its surface have been caused by the contraction of the interior through cooling.

It has further been shown that the geological phenomena require us to suppose that the crust of the earth rests upon a fluid substratum, and this belief has led to the examination, and rejection, of a second theory, (2) That the crust is thin, and is so far flexible that the fluid may rise into the anticlinals formed by the corrugations of the crust; a view held by some who have written on the subject[1], and at one time entertained by the Author him-

[1] See note, p. 169.

self. The rejection of this hypothesis has led to the supposition that the crust is flexible only to a small degree, and that, under compression, the corrugations which are raised upon the surface must be accompanied by corresponding downward protuberances of larger dimensions projecting into the fluid below, thus causing them to be duplex: and it has been pointed out that certain facts, brought to light by experiments made with the plumb-line upon the attraction of mountainous regions, have been better explained by Sir G. B. Airy upon the supposition of such protuberances, than in any other way that has been suggested. We have also shown that certain other facts of an entirely different class, connected with the observed slow rate of increase of temperature beneath mountains, are also better accounted for in this manner, than in the way originally proposed by the observer, who has studied and described the phenomena most carefully.

These two circumstances appear to lend a very considerable amount of support to this view of the subject, so that the diagrammatic sections of Chapter X., which represent the outcome of it, are thus far corroborated.

But in seeking for a cause of compression, which may have produced corrugations of the duplex character and amount required, our first attempt has failed. Here has therefore been a third hypothesis examined and found insufficient. It was (3) That the compression which has caused these corrugations has arisen from a diminution of the earth's volume through extravasation of water-substance from beneath the crust. The Author himself had some years ago suggested this idea, and it had been not unfavourably received by some leading geologists. But when he came to write the chapter in which the hypothesis is discussed, he found it, as he now believes, insufficient alone to produce the compression requisite.

All these explanations having failed, and the fact of compression still remaining patent, a fresh suggestion has been put forward, which is not intended to be more than tentative and may possibly share the fate of the rest. But it has this

advantage, that it brings the phenomena of faulting, that other form in which the disturbances of the earth's crust are manifested, into harmony with those of compression, attributing both to the same ultimate cause. The reader is however requested not to consider the suggestions last offered, regarding the cause of compression, to rest on so firm a foundation as the results arrived at concerning the existence of a fluid substratum, and of the characteristic duplex shape of the corrugations formed in the crust by compression, let the causes of it be what they may.

We will now trace somewhat further than was done in the tenth chapter, the results of denudation and of additional compression upon an elevated tract constituted thus, and compare them with natural appearances. It is evident that, as the upper and exposed part of the chain is degraded, the equilibrium will be roughly speaking restored by the whole mass being lifted up, so that the part above the effective level of the fluid should hold approximately the same ratio as before, that of $\sigma - \rho : \rho$, to the part below it. Consequently, before a mountain chain can be completely levelled down, not only must the originally elevated portion be degraded, but the crust beneath must also be lifted up and brought under the influence of the denuding agents, possibly down to the neutral zone, and the whole of the material so removed converted into sediment, and transferred to some lower level[1].

[1] To determine to how great a depth denudation might be expected to expose the crust, suppose that A is the crust of a corrugation, and B the bottom of the corresponding protuberance, Z the place of the neutral zone; and let X be the point that is ultimately brought to the surface by denudation. We have concluded that,

$$AZ = \tfrac{2}{5}AB, \text{ and } ZB = \tfrac{3}{5}AB.$$

Now if X is ultimately brought to the surface, B will at the same time be brought to the lower mean level. Hence

$$XB = c.$$

And therefore

$$\begin{aligned} AX = AB - c &= \tfrac{2}{5}AB + \tfrac{3}{5}AB - c \\ &= AZ + \tfrac{3}{5}(AB - 5c)\,; \end{aligned}$$

$$\therefore \quad AX - AZ = \tfrac{3}{5}(AB - 5c).$$

It is not however necessary to suppose that the roots of the elevated tract ever at any one time bore this proportion of $\rho : \sigma - \rho$ to the whole volume which has ultimately been denuded away; for this would not be the case unless the compression and accompanying elevation were due to a single effort. For if, after a part had been denuded off, and the roots of the mountain had been concurrently lifted up, a second period of compression had ensued, the neutral zone would then have had its position along a lower line of particles, and the depression produced might not have been so great at the second as at the first effort. The same may be said of a third, or of any subsequent effort. In fact, according to the ordinary reasoning of the method of limits, we may, by diminishing the intervals and increasing their number, pass to the case of a continuous movement of elevation, growing less and less until it ceased altogether; denudation going on all the while at a greater rate than, except at first, material was brought up for it to act upon.

If the tract were now to become free from farther compression, the sequence of events we have sketched out would be terminated by its being reduced in thickness approximately to the normal thickness of the crust in its neighbourhood. But its internal structure, as exhibited in sections, would betray the results of the treatment to which it had of old been subjected. It would form a nearly level tract, consisting of the lower parts of the folds of highly contorted rocks, which had been subjected to the action of a temperature probably above the critical temperature of water, and to great pressure.

The sediment, which had been transferred from the tract thus denuded down, would some of it have probably been spread over low lying land, and some of it have gone out to sea, and formed marine deposits not very far from the shore.

Hence if $$AB < 5c \quad AX < AZ$$
that is the neutral zone will not be brought to the surface unless $AB > 5c$.

But AB is about 11 times the height of the mountain.

Therefore the height of the mountain must have been $\frac{5}{11}c$, or say 10 miles, before denudation commenced, in order that the neutral zone should come to the surface.

Both these would have sunk nearly as fast as they accumulated, and by this means unmetamorphosed strata would have been depressed into regions where their temperature would have been gradually raised, and metamorphism set in. This would render them more dense. They must necessarily contract in consequence, and those results follow to which in the preceding chapter we have attributed compression. This compression might act directly upon the mass itself, or it might be communicated to a neighbouring tract; but on the whole it seems probable that the mass itself would be the first affected, and this agrees with the theory that compression and elevation are the consequences of the accumulation of thick deposits. The difference between the theory now offered and that proposed by American geologists consists in the cause from which the compressing force originates.

The suggestions now put forward appear to give a fairly satisfactory explanation of the causes of the deposition of series of marine strata of great thickness, of unconformity, of metamorphism, of contortion and elevation. The phenomenon which seems omitted from the explanation is that of the depression of fresh-water strata beneath the ocean, as for instance of the coal measures of various ages. This however might arise from such deposits being depressed below the level of the sea, though not under the sea, through being loaded with fresh-water sediment, as is known from borings to be going on at present in great deltas and river valleys; and then, the process being checked, the sea might cut away the upper part of the subaerial deposit, and a marine stratum might take its place. It is possible that the fresh-water deposits becoming scanty, and the district arid, faulting might be induced through contraction of the beds before the sea had time to advance, and thus we should find the fresh-water beds much broken up before the marine were laid down over them.

The exciting cause of the movements of the crust, as we have attempted to explain them, is the transference of sediment. Wherever that goes on, movements of the crust may be expected to take place. And although not altogether con-

fined to these regions, it is obvious that it is in continental areas, and along their shores that these processes are the more energetic. It is therefore conceivable that the medial regions of the great oceans may have been comparatively free from disturbance at all times. Nevertheless the growth of coral, the deposits from icebergs, and the sinking of the exuviæ of marine organisms, are causes which may contribute to the unequal deposition of sediment, over such areas as these, and we would not therefore relegate even them to perpetual repose.

It is certain that most of the existing greater mountain chains are comparatively modern; for it is possible to assign dates, geologically not remote, to the movements to which they owe their exceptional altitude. At the same time their structure usually betrays that the original effort which raised them was not the final one, for they have been subjected to repeated compression. This comparatively modern character of the greater chains leads us to believe that the gradual degradation and obliteration of a mountain range is a law of nature, in accordance with which the older chains have been levelled down and disappeared. The corrugated rocks which may have formed parts of them are still to be discovered as fundamental gneiss for instance, showing by their chemical and mechanical condition that they have been subjected at some former time to a high temperature and to great compression. They are now frequently covered up by later deposits.

The rising upwards of an elevated region by floatation concurrently with its degradation, explains some well known facts, among which are the following.

When the original contours of the strata are restored, which must have gone to form a disturbed tract, the enormous amount of subsequent denudation appears at first sight incredible. This is extremely well shown in Prof. Ramsay's restored contours along some sections in South Wales[1], where the heights and distances are laid down upon the same scale, a method

[1] "Memoirs of the Geological Survey of Great Britain," Vol. I. 1846, pl. IV.

which enables us at once to appreciate the immense amount of material that has been removed. This is not a singular case. It is a universal phenomenon. Now there is much less difficulty in conceiving strata to have been slowly lifted up from below concurrently with their degradation, than in supposing that the dry lands of old time were thousands of feet high where now we measure them by hundreds.

We have also the correlative fact of the excessive amount of sediment which an elevated tract has been found capable of yielding, and yet it remains an elevated tract still. Series of strata, five, six, or seven miles thick, and of great superficial extent, can be more easily thus accounted for, than by supposing the area from which they have been derived to have been five, six, or seven miles higher than it stands at present, even if it be now reduced below the sea-level.

The doctrine that areas must sink when loaded, and rise when relieved of a load, may perhaps explain other known phenomena. It is not impossible that the raised shell beds of Scandinavia may be partly accounted for by the country having been depressed owing to its being formerly loaded with heavy ice-fields, and that its gradual subsequent rise may have been caused by the ice having been melted off. These beaches are found up to an altitude of about 700 feet[1]. Putting three feet of ice as equivalent to one of rock, a liberal estimate, 2310 feet additional of ice would, upon the suppositions we have made respecting the relative densities of the crust and substratum, effect this amount of depression[2]. This would be

[1] Lyell's "Elements of Geology," Vol. I. p. 133, 10th edn. 1872. It may, however, be objected to this idea that several of the species in the shells' beds are of a Mediterranean type. Croll's "Climate and Time," p. 253.

[2] Let x be the height of the surface above the mean upper level when there is no ice, y the depth of the root under the same circumstances. x', y', the like quantities when there is a thickness of z ice.

Then we have the four equations,

$$y = 10x,$$
$$y' = 10x',$$
$$x' = x - 700 + \tfrac{1}{3}z, \text{ (see text)}$$
$$x' + y' = x + y + \tfrac{1}{3}z.$$

Whence $z = 2310$ feet.

less than the average thickness of the icebergs seen from the Challenger in the Antarctic ocean[1].

Similar movements have occurred, and are now going on in Greenland. Raised beaches are found up to 326 feet above the sea. But the land is now slowly subsiding at the rate of about from 6 to 8 feet per century. This may possibly be accounted for by the snowfall being at present greater than is carried off by the glaciers and by evaporation[2].

It is clear that vertical movements of the earth's crust will, according to our theory, be of two kinds; one resulting from the laws of hydrostatic equilibrium simply, and the other connected with compression. The former, although it may for a long time continue to lift up the rocks of a given area from below, can yet never by itself restore them to their pristine height. Degradation must always make more rapid progress than elevation. But the two are necessarily correlatives of each other. The degradation of the tract causes more rock to rise up, to be in its turn degraded. Such slow and continuous upward movement explains how rivers can cut across the dip of hills. It explains the drainage of such areas as the Weald, and of valleys of elevation on even a grander scale. It explains perhaps even the drainage of the great plateau of the Colorado region. It also explains the fact that we find what once were perhaps river gravels perched on the highest hills of a district. But the vertical movements arising from compression are of a different character. The effect is to elevate a tract to a higher average level than before. There may be troughs and depressions formed by it, but their amount will be more than counterbalanced by the elevations. The internal heat of the earth is the ultimate cause of this class of movements. The contraction of the interior from cooling may possibly contribute to

[1] Sir Wyville Thomson, "On the Conditions of the Antarctic," *Nature*, Vol. xv. p. 105, states that these stand about 200 feet high above water. Calculating from a specific gravity of 0·92 this gives 2,500 feet for the entire thickness.

[2] M. Johnstrup, "On the Interior of Greenland," reviewed in *Nature*, Vol. xxi. p. 344, 1880. The above passages were written before the appearance of Mr J. S. Gardner's article in the "Geol. Mag." Dec. 2, Vol. viii. p. 241, where somewhat similar views are propounded.

produce compression, and the extravasation of water-substance from beneath the crust may also play its part, but it appears that neither of these, nor yet both together, can account for an appreciable portion of the grand result. Whether the mode of action suggested in Chapter XV., more immediately derived from the same source, is capable of doing so is not easily decided. It must be remembered that the amount of compression calculated in Chapter XIII., as requisite to account for the existing elevations, refers to the whole time elapsed since they began to be formed. But the act of compression connected with the formation of the dykes beneath any given region would be confined to a comparatively limited geological period. We should therefore expect to find in them evidence of compression belonging, as regards time, only to a part of the whole amount required. On this account, ocular evidence of a smaller amount of expansion might be deemed sufficient. On the other hand, our theory localises the compressing force, and on this account we should look, as regards space, for evidence of a larger amount than if the expansion producing it were evenly distributed. Another difficulty arises, namely, that the width of the dykes will be too large a measure of the expansion arising from them, because the fissures will have been partly opened by the contraction of the rocks themselves, and only partly forced asunder by the pressure to which we are now attributing compression. It cannot be denied that, as the former three hypotheses have proved inadequate when brought to numerical tests, so, if it could be done, might also this last. Still the estimate at the end of Chapter XV. may be considered favourable. The theory possesses the advantage over the others that, if the lateral pressure be deemed sufficient, the range through which it is capable of acting is practically sufficient. These conditions are the exact reverse of the former; for in them there could be no question regarding the sufficiency of the lateral force, but the range was found to be insufficient. In the present case, the difficulty consists in applying any test to see whether the phenomena indicate that the force now appealed to can in fact have acted through a sufficient range to have produced the requisite compression.

CHAPTER XVIII.

MR MALLET'S THEORY OF VOLCANIC ENERGY.

Early opinions regarding the seat of volcanic energy—Hopkins' lava lakes—Mallet's theory—Opposed to the results of the present work—His mode of estimating the temperature derivable from crushing rock—His results published in the "Phil. Trans."—differ from those given in an earlier publication—Latent heat of fusion—Localization of heat from crushing not possible—More favourable hypothesis suggested and considered—A pressure, the greatest obtainable on the hypothesis, incapable of causing volcanic phenomena.

It has been taken for granted more than once in the preceding pages, that the seat of volcanic energy is situated in the fluid substratum, which we have endeavoured to prove must underlie the cooled crust of the earth. This is the natural supposition, and was formerly generally assumed by geologists. But when Hopkins published his supposed proof of the great thickness of the earth's crust, he was constrained to offer a fresh explanation of this class of phenomena. Hence his theory of subterraneous lakes of lava. When Sir Wm. Thomson corroborated Hopkins' view, and carried it further to the extent of asserting the entire solidity of the globe as a whole, the old assumption concerning the universal distribution of fluid matter beneath the crust received a further blow. The theory of Hopkins' lakes was permitted to stand, but it was felt to be so improbable, that physical geologists in general could not rest satisfied with that explanation.

At this juncture Mr Mallet published his theory of volcanic energy[1], which at the time of its publication was received with

[1] "Phil. Trans. Royal Soc." Vol. 163, p. 147. 1873.

considerable favour by many. Impressed with the necessity of admitting the doctrine of a solid earth upon the authority of the highest masters of physical science, they saw in Mr Mallet's hypothesis a way of escape from the difficulty in which they were placed. If the earth be a solid body cooling by conduction, and if the corrugations upon its surface are caused by the contraction of the mass and consequent compression of the superficial layers, what, it has been thought, can be more in accordance with probability, than that the work of compression should be converted into heat, and that volcanic energy should be its manifestation, and have its seat no deeper than the compressed crust. Like many simple explanations, the only wonder was that no one had suggested it before. Yet how often had been long delayed the discovery of some theory, which when once proposed seems simplicity itself.

If Mr Mallet's theory be true, it is obvious that that now propounded must be untenable. It will therefore be necessary to examine it.

The theory is enunciated in the following terms[1]:—

"*The heat from which terrestrial volcanic energy is at present derived is produced locally within the solid shell of our globe by transformation of the mechanical work of compression or of crushing of portions of that shell, which compressions and crushings are themselves produced by the more rapid contraction, by cooling, of the hotter material of the nucleus beneath that shell, and the consequent more or less free descent of the shell by gravitation, the vertical work of which is resolved into tangential pressures and motion within the thickness of the shell.*"

As regards the lateral pressure to which Mr Mallet appeals as the cause of compression, and of crushing portions of the shell, there can be no doubt that, if there were a continual contraction of the interior and subsidence of the shell going on, the pressure arising from the descent of the shell would be perfectly adequate to produce the crushing attributed to it.

We have however, as we believe, seen grave reason to doubt

[1] *Op. cit.* § 67, p. 167.

whether this contraction exists, at least to such an extent as to produce any appreciable movements of the crust during such periods of time as history embraces. Yet volcanic phenomena are of daily occurrence. This appears to dispose of the theory *in limine.* Nevertheless it is possible that this argument against it may not meet with general acceptance, and it is therefore desirable to examine Mr Mallet's theory upon his own assumptions.

No one can study the paper, in which the theory is propounded, without admiring the amount of knowledge displayed in it, and the numerous and laborious experimental investigations which he undertook to form the basis of his theory. The records of these alone will render his work of lasting value. Nevertheless we cannot avoid the conviction, that they do not warrant the conclusions which he has drawn from them.

The experiments which form the foundation of the theory may be shortly thus described:—

Cubes of rock of sixteen different kinds, varying in respect of hardness from soft oolite to hard porphyry, were carefully cut into cubes of 1½ inches on the edge. These were then crushed by means of a powerful lever, under a cylindrical piston or plunger of 3½ inches diameter. The cubes of rock appear not to have been confined at all laterally, so that, when they gave way, they were compressed into a cake of powder beneath the plunger.

The pressure upon each square inch of the face of the cube was calculated from the accurately known pressure laid upon the plunger, and the vertical descent of the plunger, while the crushing was going on, was also carefully measured. These multiplied together gave the "work" of crushing, and by dividing the work so obtained by Joule's equivalent the corresponding quantity of heat was calculated, it being assumed that all the work was transformed into heat.

If we consider the summary of the series of experiments in crushing cubes of rock[1], it is evident that the vertical descent

[1] *Op. cit.* Tab. I. column 19, p. 187.

of the plunger must have been rendered much greater by the cube of rock having been free to fall asunder on all sides. If it had been confined in a box of its exact size it might still have been crushed; but the amount of descent would have been in that case dependent solely upon the increase of density which could be given to it after disintegration by the pressure. If the box had been somewhat larger, the plunger would have descended further, and if the rock was altogether unsupported, as seems to have been the case, further still. The value of H (the heat) found in accordance with the experiment is correct; but the form of the experiment cannot represent at all closely what would happen deep in the earth's crust. It seems that the cubes should have been confined, that the experiment might more closely represent the case of nature.

But accepting the results as given, the equation connecting the quantities involved may be thus arrived at. Let

$W =$ the pressure laid upon the plunger.

$h =$ the height through which the plunger descended.

$J =$ Joule's equivalent, or the number 772.

$H =$ the number of units of heat into which the work of crushing was transformed.

Then $$H = \frac{Wh}{J}.$$

Also, $t =$ the temperature through which the rock was raised by the crushing.

$s =$ the specific heat of the rock, *i.e.* the number of units of heat requisite to raise 1 lb. of rock through 1° F.

$$= \frac{\text{quantity of heat requisite to raise } w \text{ of rock } 1^\circ}{w}.$$

$$\therefore \quad ts = \frac{\text{quantity of heat requisite to raise } w \text{ of rock } t^\circ}{w}$$

$$= \frac{H}{w}.$$

Hence $$t = \frac{H}{sw}.$$

This is Mr Mallet's equation (6), § 102, where he takes w to be the mass of one cubic foot of rock, and of which we have for clearness given a demonstration. His experiments gave for the mean value of s the number 0·199; and for the mean weight w of a cubic foot of rock 177 lb. The mean number of British units of heat developed by crushing one cubic foot of the harder rocks is estimated by him at 5650; and it appears upon calculating the value of t, that the mean temperature by which a cubic foot of such rock would be so raised is 172° F. Or if we take the particular kinds of rock selected by Mr Mallet (§ 133), these means are found by him to be 6472 and 183°·74 F. And if the rock was previously at 300° F., taking 2000° as the fusing temperature, he finds 0·108, or rather above one-tenth, as the fraction of a cubic foot of rock which the heat developed by crushing *one* cubic foot of rock could fuse. Or, to put it otherwise, it would require the heat developed by crushing *ten* volumes of rock to fuse about *one*.

Here we meet with a remarkable discrepancy between the results given in the paper in the Philosophical Transactions and those previously given in the introductory sketch to the same author's translation of Palmieri's "Vesuvius[1]," for it is there stated that using the mean of 6472 units of heat as derived from crushing one cubic foot of rock "each cubic mile of the mean material of such a crust, when crushed to powder, developes sufficient heat to melt 0·876 cubic miles of ice into water at 32°, or to raise 7·600 cubic miles of water from 32° to 212° F., or to boil off 1·124 cubic miles of water at 32° into steam of one atmosphere, or, taking the average melting point of rocky mixtures at 2000° F., to melt nearly three and a half cubic miles of such rock, if of the same specific heat." It is obvious that it makes no difference in the ratio, whether the unit be a cubic mile, or a cubic foot, or a cubic inch. So that the statement here amounts to saying, that the crushing of one cubic foot would melt $3\frac{1}{2}$ cubic feet, instead of 0·108 of a cubic foot; or more than 32 times as much as subsequently stated in the

[1] p. 69. There is a foot-note to the paper in the Transactions which speaks of a modification of § 102. This may be connected with the discrepancy referred to.

Transactions. Indeed the experimenter must have felt surprised that the cubes were not melted, seeing that the heat developed ought to have been three and a half times as great as necessary for the purpose.

It will also occur to the reader, that Mr Mallet has not made any allowance on account of the latent heat of fusion, but has assumed that, when sufficient heat had been supplied to a mass of rock to raise it to the temperature of fusion, it would become wholly fused. This of course is not the case, because much of the heat would be employed in producing the change of state from solid to liquid, without producing any effect on the temperature.

But accepting the conclusion that the heat resulting from crushing one cubic foot could fuse 0·108 of a cubic foot, since this is not capable of fusing the whole of the cubic foot crushed, Mr Mallet considers that this heat may be localized, and that the heat developed by crushing ten cubic miles of rock, may fuse one mile. But it may be conclusively shown that this is not possible; for let us consider a horizontal prism of rock of any length. This is itself a part of the earth's crust, and by its rigidity has, up to the moment of its giving way, resisted, and so permitted, the necessary accumulation of the pressure which eventually causes it to yield. Conceive that in this prism there are portions situated here and there which are weaker than the average, and that these weak portions when crushed allow of the prism being shortened at the places where they are situated by the quantities α_1, α_2, α_3, &c. respectively. It is clear that the weaker places will yield first and under a less pressure, and by the relief so afforded delay the crushing of the others, because the pressure must accumulate afresh. But, for argument's sake, we will suppose all to yield together, and the pressure throughout the action to be equal to the value it had at the first yielding.

Now suppose that when the prism has yielded the whole of it becomes shortened by the length a. If, then, P be the pressure which caused it to yield, Pa will be the whole work done upon the prism. The length a is made up of the portions

α_1, α_2, α_3, &c., by which the weak portions have been shortened; while $P\alpha_1$, $P\alpha_2$, $P\alpha_3$, &c. are the portions of work done at these places. And these taken together make up $P\alpha$, since

$$\alpha_1 + \alpha_2 + \alpha_3 + \&c. = \alpha.$$

We see, then, that the work must be confined to these places; for if there were work done elsewhere we should have more work than $P\alpha$, which is impossible. Hence the work convertible partially into heat takes place at all these places, and at each in proportion only to the yielding, and nowhere else; so that it cannot be localized at any one place. We may then conclude that, unless the heat got out of crushing any portion of rock is sufficient to fuse that particular portion, none will be fused. Indeed we may go so far as to assert that, if Mr Mallet's experiments give the amount of heat which can be obtained by crushing rock, and the heat so obtained can fuse it, then the cubes experimented upon ought to have been fused.

But it is scarcely satisfactory to leave the question at this point. It is conceivable that the pressures, which were experimentally applied to the cubes of rock, might be far exceeded within the earth's crust, if compression were caused there by the contraction assumed in the theory under discussion; which it must be recollected is quite opposed to the results obtained in the preceding Chapters. Let us then survey the supposed conditions of the problem. A contracting globe induces in its enveloping crust a state of compression, which owes its existence to the gravitation of the whole towards the centre of the figure. The interior goes on contracting until the compressing force accumulates sufficiently to cause a movement of some kind among the particles of the crust, be it crushing, or faulting, or corrugation, or what not; and the motion being arrested, the work becomes transformed into heat. This heat, as already proved, can be developed only at those places where the movement occurs. The question is whether it would be sufficient to give rise to a volcano. If some places are weaker than others, the movement would necessarily occur at them. Let there be several such, which for simplicity we will suppose to be situated at points upon a great circle. The force of compression will go

on increasing until one of these places gives way. Let P_x be a general symbol for this increasing horizontal force, measured by the pressure, in terms of the weight of a cubic mile of mean rock at the surface, upon a square mile of vertical section of the crust.

Now it is evident that no greater lateral shortening, or approach of the particles of the crust among themselves, can take place at one place than at another, except through a lateral movement of the layers of the crust over the nucleus towards the place in question. Hence any localization of work at a given place must require this to occur more or less. Consequently if the pressure goes on accumulating until it becomes equal to P_x all round, and under that pressure the crust begins to yield at A, the yielding there cannot relieve the pressure anywhere else, unless the crust is shifted over the nucleus towards A.

Having premised thus much, we will endeavour to find the utmost amount of heat which could be developed along a vertical section at any one place in the crust.

Conceive a strip of the crust, one mile in width, situated along a great circle; and suppose the pressure to have gone on increasing to some value P_x, which has caused the crust to yield

at A; and that by such yielding the pressure has been reduced to P_A at A, and partially relieved over AB and AB' on either side of A, but not just beyond B and B'. Hence at B, B' the pressure will still be P_x. Somewhere beyond B and B' it will have been relieved by yielding at other places; and it is supposed that it does so yield all round, so as to fit the reduced nucleus. Hence the amounts to which the pressure will accumulate at different places will depend upon the strength of the crust; but the amount of *shortening* of the whole crust will not depend upon the amount of the pressure, but solely upon the amount of contraction of the nucleus. Consequently if e be the coefficient of linear or radial contraction, seeing that the points

B, B' are not shifted upon the nucleus, it follows that the compression of BB' is eBB'.

Let μ be the pressure which a unit in length of the crust would just resist, so as under its action not to move over the nucleus. Then, before motion commenced, μ would depend on the adhesion, but after motion had commenced, on friction; and μAB is the force which the length AB would just resist. Let k be the thickness of the crust. In general μ will not be the same for BA and $B'A$; but we will suppose it so, in which case BA and $B'A$ will be equal. Hence, after compression has taken place, we shall have for equilibrium, at sections passing through B and B',

$$P_X k = \mu AB + P_A k,$$

and

$$P_X k = \mu AB' + P_A k.$$

Adding

$$2P_X k = \mu BB' + 2P_A k,$$

whence

$$BB' = 2\frac{P_X - P_A}{\mu}k.$$

Therefore the compression between B and B', which is supposed to be localized at A, being eBB', we have,

$$\text{compression at } A = 2\frac{P_X - P_A}{\mu}ek.$$

Respecting the force which has acted at A to give rise to this compression, we observe that it was $P_X k$ when the compression began, and $P_A k$ when it ceased. We may therefore put it at their mean, or $\dfrac{P_X + P_A}{2}k$. Hence the work at A, which is the product of these two quantities, $= \dfrac{P_X^2 - P_A^2}{\mu}k^2 e$.

We will now find a limit which must exceed the greatest value that the above can reach. It is evident that this will be given by assigning to P_X the utmost value of the compressing

force[1], viz. the weight of a column of rock 2000 miles high and 1 mile in sectional area and of the density of the crust, which we will term P, and by giving to P_A the value zero.

A superior limit to the whole work on a vertical section of the strip of crust will therefore be

$$\frac{P^2}{\mu} k^2 e;$$

and upon a square mile of this section,

$$\frac{P^2}{\mu} ke.$$

Mr Mallet, at p. 8 of his paper in the "Philosophical Magazine" for July 1875, states that the friction may be as great as $\frac{3}{4}$ of the pressure. Let us, then, suppose first that the nucleus is solid, and that the force requisite to move a column of the crust horizontally over the nucleus is $\frac{3}{4}$ of the weight of the column; and P is the weight of a column 2000 miles long. Hence

$$\mu = \tfrac{3}{4} P \frac{k}{2000};$$

whence, if $k = 400$ miles[2], which is the thickness suggested by Mr Mallet,

$$\mu = \tfrac{3}{4} \frac{P}{5}.$$

Hence the work upon a square mile of section cannot be so great as

$$\tfrac{4}{3} P 5ke,$$

which

$$= \tfrac{4}{3} Pe \times (2000 \text{ miles}).$$

To assign a value for P, we use the weight of a cubic foot of granite, given by Mr Mallet as 178·3392 pounds (say 200 pounds). And for the supposed value of e, which we may obtain from the radial contraction during one year as given by the same Physicist in his *Addition*, &c., Tab. II.[3] for 400 miles thickness of crust,

$$e = \tfrac{9}{4} \frac{1}{10^{12}}.$$

1 p. 36.

2 We must recollect that the crust is assumed to be thick.

3 "Addition to the paper on Volcanic Energy." "Phil. Trans. Roy. Soc." Vol. 165, Part I.

Hence we find for the work for one year,

$$\text{work} = 76908 \text{ foot pounds};$$

and dividing by Joule's equivalent, 772;

$$\text{heat per square foot} = 99 \text{ units}.$$

The corresponding value for BB' will be 5333 miles.

Now, according to Mr Mallet's results in his principal memoir, § 133 (1) and (6), we are informed that 6472 units of heat would fuse 0·108 cubic foot of mean rock previously at 300° F., taking 2000° as the fusing temperature. Hence with a solid earth, according to our calculation, *a limit greater than the greatest possible* gives 0·0015 cubic foot of mean rock that could be fused annually for each square foot of the plane of vertical section. With this value it would take a thousand years to fuse *one* vertical slice of a foot and a half thickness within the range of crust defined by the distance BB', or 5333 miles, on a great circle of the sphere. But the place of weakness cannot but have a considerable width throughout which the heat would be distributed. If, then, it were, say, a mile wide, the number of units of heat to each cubic foot would be $\frac{99}{5280}$, or about 0·02 unit.

The force of compression, besides the work of crushing at A where the yielding takes place, also does work over the plane of shearing in overcoming adhesion and friction. Let the whole force expended in overcoming adhesion and friction along BB' be $\mu' BB'$. The space moved over increases from nothing at B and B' to eAB and eAB' at A. The mean is $e\dfrac{BB'}{2}$. Hence the work along this plane is

$$\mu' BB' \times e\frac{BB'}{2}$$

$$= 2\mu' \frac{P_X - P_A}{\mu} k \times e\frac{P_X - P_A}{\mu} k.$$

And making the same suppositions as before for a superior limit, this becomes on the whole plane

$$2\mu' \frac{P^2}{\mu^2} ek^2.$$

We took μ at the value $\frac{2}{3}$ of the weight for friction. We will take it as equal to the weight for the adhesion, since that is probably greater than the friction, although its ratio to the friction probably diminishes as the pressure increases. Hence

$$\mu' = \tfrac{4}{3}\mu,$$

$$\therefore \text{ the work} = \tfrac{8}{3}\frac{P^2}{\mu}ek^2.$$

But the whole work on a vertical section was found to be $\frac{P^2}{\mu}ek^2$; hence the whole work along the plane of shearing is $2\frac{2}{3}$ of the work along the vertical section. This work will not be evenly distributed, but will be greatest upon the unit of the length nearest A, where the space moved over will be eAB, and the force upon it is μ'. Hence work upon the last unit $= \mu' eAB$,

$$= \mu' e\frac{P_X - P_A}{\mu};$$

upon the suppositions made, this becomes

$$= \tfrac{4}{3}Pek.$$

And the work upon a square mile of section on the same suppositions was found to be

$$\tfrac{4}{3}P5ek.$$

Hence the work, and therefore the heat, upon the last unit of the plane of shearing, where it will be greatest, is only one-fifth of the work and heat respectively on a unit of the vertical section.

This result seems unfavourable to the view that the heat arising from friction of the rocks along horizontal planes can account for the heat concerned in producing even metamorphism.

It must be fully understood that the results just arrived at with respect to the heat developed at a vertical section, as well as along the plane of shearing, have reference not to any *probable* values, which under the assumed conditions could arise, but to a *superior limit* which must be greater than the greatest possible. In fact they involve several impossible assumptions, all of which make it too large. These are:—

(1) That the contraction from cooling would take place in the matter of the nucleus and not in the crust, for this supposition is involved in Mr Mallet's value for the contraction.

(2) That the above circumstance would allow the coefficient of contraction to be nearly four times as large as on the more probable supposition.

(3) That the pressure increases to the utmost attainable value, viz. that of a column of rock 2000 miles long, before yielding takes place.

(4) That when yielding does take place, the pressure at the place of yielding is entirely satisfied.

The above assumptions, besides making the amount of heat impossibly great, also localize it to an impossible degree, as may be seen by observing that BB' would be lessened by diminishing P_X, and also by increasing P_A; that is, by lessening the pressure which causes the yielding, and by increasing the pressure at the place of yielding after the compression has taken place.

The calculations indicated above will be found given at greater length in the Author's paper on this subject in the "Philosophical Magazine[1]," together with an extension of the investigation to some further hypotheses as to the crust sliding over a fluid nucleus. But it is thought unnecessary to reproduce more here to show that the theory does not account satisfactorily for volcanic phenomena.

We see then that Mr Mallet's own experimental mode of estimating the pressure requisite for crushing the strata cannot give rise to sufficient heat for the purpose; and that if we go far beyond this, and use in forming our estimate the utmost pressure under the circumstances conceivable, we still fall far short of obtaining the requisite amount of heat. Accordingly, we are

[1] Fourth series, Vol. 50, p. 302, 1875 (in which occurs the following erratum. At p. 316, line 6, insert k, for "$1\frac{1}{3}$ mile" read "533 miles," and *dele* the following line). See also Mr Mallet's reply, fifth series, Vol. I. p. 19, and the Author's rejoinder, *Ibid*, p. 138.

thrown back upon the old explanation of volcanic phenomena assumed in the preceding pages to be true, that they are a manifestation of the intense temperature existing at great depths, and that the fused rocks are conveyed to the surface through channels of communication.

CHAPTER XIX.

THE VOLCANO IN ERUPTION.

Theories of volcanic eruption—Scrope, Dutton, Mallet—Local and temporary increase of temperature not probable—Richthofen's theory unsatisfactory—Explanation of phenomena on hypothesis of fluid substratum in igneo-aqueous fusion—Amount of water-substance beneath vent probably variable—Mechanics of an eruption—Estimate of amount of water-substance in magma—Eruption, how brought to an end—Evisceration of cone—State of intermediate activity—Stromboli—Unsympathetic craters—Richthofen's difficulties met—Renewal of activity—Comparison with Geyser.

THE theory of the existence of an intensely hot substratum of rock in a state of igneo-aqueous fusion, underlying a crust of about 25 miles thickness, seems on the whole to offer the conditions calculated most readily to explain volcanic phenomena. The alternative view may fairly be stated to consist in the hypothesis that periodical local increments of temperature occur beneath volcanic areas, temporarily fusing the rocks, and causing their eruption. This was Scrope's opinion[1]; Captain Dutton holds the same view, viz. "that the proximate cause of eruptions is a local increment of subterranean temperature, whereby segregated masses of rocks, formerly solid, are liquified"[2]. Neither of these writers attempts to explain the cause of the supposed increment of temperature. Mr Mallet's theory, discussed in the preceding chapter, is of the same kind, except that he proposes to account for the local and periodical elevation of temperature by mechanical means. In whatever way

[1] "Volcanoes," p. 263.

[2] "Geology of the High Plateaus of Utah," p. 120. Washington, 1880.

we regard it, the hypothesis of a local and periodical heating of the rocks appears to be surrounded by great difficulties. Such a rise of temperature can be produced only in one of three ways; (1) by mechanical work being locally converted into heat, or (2) by local chemical reaction, or (3) by heat being transferred from some hotter neighbouring region. The first is Mallet's hypothesis, and is believed to be insufficient. The second was Davy's, and has been long abandoned. There remains only the third. Now there are but two ways in which heat can be in such a case conveyed from hotter to cooler regions, and these are by convection and by conduction. If the former be the mode appealed to, then we have necessarily a fluid condition of the underlying rocks, which is our hypothesis. But if conduction be the process involved, then we must have an unfused mass, beneath, and adjoining, the area affected, at a temperature higher than is requisite to fuse other rocks; which is in itself improbable. But that such a condition of excessive temperature should be local and temporary, is still more so, because the tendency of conduction would be to equalize the temperature along isogeotherms approximately parallel to the outer surface.

There is however, the theory of Richthofen to be considered. Instead of invoking a rise of temperature, he supposes a temporary increase of fusibility. He appears to think that there are heated couches of rock, in a condition which he calls "glowing lava[1]," but which are not in a state of fusion. Fissures being formed in the crystalline crust above this, water gains access to it, and it enters into a state of "aqueous fusion," and thereby becomes increased in volume, and "this expansion would immediately cause a motion of the masses rendered liquid, in the direction of least resistance, that is, upwards in the fissure, and would, if continued for a sufficient time, make the same overflow on the surface of the crust, even if unassisted by other ejecting agents, such as the vapour of water[2]."

The difficulties attending the accession of superficial water

[1] "Natural system of Volcanic Rocks," p. 66. [2] *Ibid.* p. 56.

to the volcanic foci have already been pointed out[1], and it seems hard to believe that the mode of action here described can have a real existence. Moreover Richthofen appeals to the cooling of the globe as the ultimate cause, of which cause we claim to have proved the inadequacy. He says, "elevations and eruptive activity, even when locally not quite coincident, are co-ordinate effects of the cooling of the globe; but while the one is its immediate effect, the other results from it only by the concurrence of other agencies, which by themselves alone would have been incapable of producing results of such magnitude." The memoir from which these passages are taken is of great interest, and contains many very suggestive observations, and sage generalisations well worthy of study. On the whole his theory of volcanic action approaches more nearly than any other to that which we now advocate.

We have already indicated in a general way the manner in which we would explain the phenomena with such a crust as we have supposed, subject to disturbances from transference of sediment, and consequent local depression. The contraction of the lower parts of the crust, where it becomes depressed into regions of higher temperature, will induce metamorphism, and shrinking, and shrinking will cause fissuring. Changes in the distribution of load by transference of sediment will also have a tendency to produce fissures, and it has been pointed out how these, commencing below, may be propagated until they reach the surface, and become filled with lava, in which the expansion of the water-substance, at a temperature far above the critical point, will render the column of molten rock on the whole specifically lighter than the mean crust, so that it will escape at the surface.

In a general way this seems probable, but we have already shown that, under certain admissible suppositions, what we have just stated will necessarily occur. It is true that we have considered the problem only as a statical one, for the ascent of vesicles of water through the lava, causing ebullition at the surface, has not been taken account of, but merely the decrease in density of the lava upwards, caused by the expansion of

[1] p. 90 et seq.

the water-substance in it through decrease of pressure, the same identical water being supposed to remain always engaged in the same portion of lava[1]. But ebullition would help to raise and eject the lava.

In the equation which we have arrived at, connecting the pressure p of the lava at the level of the surface of the crust with the proportion $1 - V$ of water-substance, which the magma contains as it exists below when subject to the pressure of the crust, if we assume a value for one of these quantities, we can find the other. The equation in question is[2],

$$\frac{1-V}{V}\log\frac{g\rho k}{p} - \frac{p}{g\rho k} = \frac{\sigma}{V\rho} - 1.$$

Probably in different localities, and at the same locality at different times, these quantities, p at the surface, and $1 - V$, are different; and it is to the connection between the quantity of water and this pressure that the well-known fact is due, that a long continued state of repose in a volcano is followed by an exceptionally violent eruption, and *vice versâ*. In order that the pressure at its base may bring the lava to the surface of the crust, it is requisite that the mean density of the column should be lowered by a certain amount of water being vesicularly mingled with it. But the vesicles rise through it, and eventually leave the column of lava deficient in enough water-substance to enable it to be raised to the surface. At the same time the magma around the base of the funnel has been to a certain extent exhausted of its superabundant water, and thus an eruption will have the effect of diminishing the proportion of water in the magma, or $1 - V$. Exceptionally powerful eruptions might be expected to occur when a volcano had been quiescent for a long while, and the water had become again equably diffused, and regained its average proportion, beneath the vent; or when a volcano has newly burst through the crust at a place where the water-substance had never, or at least had not for a long period, been drained away. We have of course no means of knowing with any degree of certainty the values of either of these quantities, p at the surface, or $1 - V$.

[1] p. 189. [2] p. 191.

There is however to guide us the consideration, that the quantity of water, which is given off along with the lava, will bear a ratio to the quantity of the lava depending upon the value of V. If it be true, as we have seen reason to think to be the case, that the water-substance in the lava must be of the same density as the rocky matter with which it is combined, then the proportion of water-substance to rocky matter being $1 - V : V$, the proportion of ordinary water to the lava in the erupted matters will be nearly $\sigma (1 - V) : V$. Thus if V were 0·9, or the compressed water-substance occupied one-tenth of the volume of the magma, and if σ were 2·96, the water given off would bear to the lava given off the ratio of $2{\cdot}96 \times 0{\cdot}1 : 0{\cdot}9$, or about 0·3 : 1; that is the volume of the lava stream would be about three times the volume of the water; which does not appear improbable.

When we take into account the passage of the vesicles of water-substance through the lava in their ascent towards the surface, we cannot doubt that this takes place with greater rapidity than the ascent of the lava itself, and the exhaustion of water in the column goes on much more rapidly than diffusion can repair it. Consequently the lava in the upper part of the column at last becomes so deficient in vesicles of water-substance, that its weight balances the expansive force beneath it. The vesicles beneath it will then cease to expand, and their upward motion will be checked, and the evolution of steam gradually almost cease. But the column of lava has been for some while rising in the vent, and at this juncture the forces which press it upwards are just in equilibrium with those that keep it down. The *vis viva* which it already possesses will be only gradually destroyed, and it will continue to well forth steadily and without much evolution of vapour for a little while and then sink back into the vent. In this manner the eruption will be brought to a close[1].

[1] Instances are recorded in which an eruption of lava has been followed by a great evolution of steam and ashes. It is conceivable that vapour may accumulate in parts of the chasm which are closed above, and when the level of the lava which confined it there has sunk sufficiently, steam may find an exit by the open vent, rushing with violence through the lava, now no longer deep enough to restrain its escape, and blowing it into dust; the mode of action being some-

The ultimate cause of its cessation has been the exhaustion of water-substance in the column of lava. It is not by any means requisite however, that the whole of the water-substance shall have been exhausted, or that none shall be passing through the column which still remains in the funnel. This is seen to be the case, because much vapour is continually given off by volcanoes when they are not in eruption, though the quantity is incomparably less than what is given off when they are so.

According to the formula we have made use of to express the density of the lava, we find that if it rose in the vent, containing in its composition the same amount of water with which it started from below the crust, its density at the level of the surface of the crust would be little above that of ordinary water; for

$$\mu = \frac{\sigma}{V + \frac{g\rho k}{p}(1 - V)},$$

and giving to V the value 0·9 and to $\frac{g\rho k}{p}$ the corresponding value at the surface, which, with $\sigma = 2{\cdot}96$, we have found to be 14·4, we obtain for μ the value 1·26. And when a volcano is in full eruption, and immense volumes of superheated water are passing upwards, it does not seem at all improbable that the mean density of the lava at that level, which will be at the base of the mountain, may be as low as this. Such a low density would agree well with the evisceration of a cone during a great eruption, so well described by Scrope[1]. We may conclude that a large supply of water is requisite for such manifestations of activity.

If so much water must be present in the column to produce an active condition, we can understand how moderate quantities of superheated water passing upwards, although insufficient to cause eruption, may nevertheless be enough to keep the column of lava, or at least its axis, in a state of fusion. But if the

what similar to that which was proposed to explain the phenomenon of a geyser, before Bunsen suggested a better.

[1] "Volcanoes," p. 186.

means of supply of water-substance be more liberal, and be just balanced by the means of escape, a gentle and perpetual state of eruption like that of Stromboli, may result. That the maintenance of the temperature of fusion in a habitual vent is due to the cause suggested, appears probable from the following consideration. Molten lava sometimes fills the bottom of a crater for a long period, its surface being maintained in a constant state of ebullition by the vapour and gases which pass through it. In such a case it must be the high temperature of these vapours which keeps it melted: for if the fusion were due to a continual supply of fresh lava from below, there would need to be a continual escape of it above. But this does not appear to be requisite. If such be a correct view of the case, it shows that the hot vapours are capable of fusing rocky matter, and accordingly, when once they have obtained a passage of escape, they may convert the sides of the channel into lava and fuse their own way through, carrying the fused matter with them[1].

It has been often urged that volcanic vents cannot communicate with a common reservoir below, because, even in the same mountain, contiguous craters are not sympathetic. But this sympathy does not seem really necessary, for as soon as a column of ascending vapour has established itself in one crater, the tendency will be for it to continue to hold the same position, since its presence renders that the vertical of least pressure, and consequently the channel of easiest escape. We see the same sort of thing take place when water begins to boil in a vessel heated below. Before it passes into general ebullition, a column of bubbles will appear here and there, and rise from the same points of the bottom of the vessel for a considerable time. So, when the escape of vapour has commenced through one of the craters, it may carry off the vapour as fast as it is supplied, and determine the eruption to that channel, the neighbouring crater remaining quiescent, although they may both of them be in free

[1] See the author's paper "On the Inequalities of the Earth's Surface upon the hypothesis of a liquid substratum." "Camb. Phil. Trans.," Feb. 1875.

communication with the same subterraneous reservoir[1]. The level of the lava in the crater which is in eruption will be raised above that in the other, because the column beneath it is rendered lighter by the expansion of the increased quantity of superheated water which is passing through it at the time. The case will be somewhat like that of an inverted syphon, the liquid in one leg of which is lighter than that in the other.

The explanation now offered for the elevation of the lava to the surface, differs from previous theories of the same kind, on account of the supply of water-substance being drawn from the entire liquid substratum, and not from the access of it to the lava at some point higher up. The importance of this distinction appears from an objection made by Richthofen[2]. "It must here be remarked that some of the most eminent writers on the subject of volcanoes, chiefly Poulett Scrope, Prof. Dana and others, have suggested long ago, that the ascent of lava in a volcanic channel must be due to both the fluidity and expansion imparted to it by the globular state of the water which finds ingress to the channels of the lava and enters into its composition. They have anticipated, by this suggestion, in some measure, the results of experiment established by Daubrée. But the supposition was only made for the case of lava, and not for that of rocks which were ejected without volcanic action proper" (that is by massive, or fissure eruption). "Yet even in regard to volcanoes it did only explain the extension of lava to the surface from a place at a limited distance below it, and failed to give a clue to the manner in which the constant supply of matter to those places was kept up"[3]. Both of these objections appear to be met if the entire liquid substratum is laid under contribution for the supply of the water-substance which acts as the intermittent agent in raising the column to the surface.

[1] Another explanation suggested has been that the craters, though contiguous, may be connected with different fissures.

[2] "Natural system of Volcanic Rocks," "Memoirs of the California Academy of Sciences." Vol. I. Part II. 1868.

[3] *Ibid.* p. 54, note.

The mode of action suggested appears competent to account easily for the eruption of steam and lava in a case where a channel of communication has been freshly opened between the liquid substratum and the surface. There is more difficulty in understanding how, when an eruption has taken place, and the water-substance has been exhausted to that degree at which the lava becomes too dense to rise any longer, the action can be after a period renewed. And as there is a difficulty in understanding the process, so also is there evidently a difficulty in the process itself, otherwise the periods of repose would not be so long and so irregular as they are, and sometimes perpetual.

The comparison between the geyser and the volcano is no new one. Bunsen's explanation of the former turns upon the temperature at each depth in the column of water becoming raised above the boiling point for the pressure there. Our account of a volcanic eruption is somewhat similar, except that the boiling point of the lava depends, not so much on the temperature, which we have regarded as constant (although that cannot be strictly so), but upon the presence of a sufficient quantity of water mingled with the lava, to cause the requisite expansion of volume under the pressure at each depth. What will be required therefore to repeat the eruption, will be the diffusion afresh of sufficient water into the column for this purpose, and it seems that the escape of vapour from the surface becoming reduced below the supply gained by diffusion from below, must be the requisite condition. This may be perhaps caused by the cooling of a plug of lava at the top of the column, except a narrow passage through which what escape there is takes place, and which serves as the leading channel for the next eruption. Or else, when the period of repose has been protracted until this channel has become blocked with cooled lava, some fresh movement of the crust may open the necessary communication between the surface and the fluid substratum.

CHAPTER XX.

SEQUENCE OF VOLCANIC ROCKS.

Flow of matter melted off roots of mountains—Force causing it considerable—Formation of lavas—These will differ in different areas and at different times—Law of Bunsen—Richthofen's "Natural System of Volcanic Rocks"—How explained by him—Difficulty arising—Captain Dutton's explanation—Prof. Judd on Richthofen's System—Melting of roots of the mountains affords an explanation of the sequence—And of the variation of volcanic products in adjacent regions.

It has been already remarked that, where the roots of the mountains project downwards into the hot fluid substratum, they must be slowly and gradually becoming fused[1]. The material that becomes fused is derived from the crust, which is of less specific gravity than the substratum, and it must be likewise remembered that the roots of the mountain extend downwards to a considerable depth, perhaps 10 times the height of the upper mountain, into the substratum, which will be more dense in its lower layers. The consequence of this must be, that the fused rock which is melted off the roots will have a tendency to flow upwards, and to accumulate along the under side of the crust much in the same way that lava, coming from a vent, runs down a mountain's side, and spreads itself out upon the surface. We must in the present case, as it were, invert our ideas of the usual movement of a liquid mass. The fused rock which runs up these roots will not be cooled as subaerial lava is, but will retain its liquidity. On the other hand, moving in a medium about as dense as itself, its motion will be very

[1] p. 152.

slow, and there will be a tendency for the two fluids to become mingled in varying proportions according to circumstances. If the fused rock creeps along the top, itself in contact with the under side of the crust from which it has been derived, its composition on the upper side of the flow will be little altered, but on the lower side it will be more or less mingled with the substratum, owing to the friction between the two.

There can be no doubt that there will be sufficient heat in the substratum for the above purpose, because the magma, even after the water which it contains has been blown off in vapour, and so carried away a vast amount of heat, retains sufficient heat to afford liquid lava. There must therefore be heat to spare in the substratum to melt matter off the roots without itself becoming congealed.

It will be desirable to compare the force which urges the fused rock up the inclined root, with that which would urge a liquid down the incline at the point vertically above it on the surface of the mountain.

Let θ be the angle of inclination to the horizon at a point on the side of the mountain at a height h above the mean level, and let θ' be the angle of inclination to the horizon at the point vertically beneath it upon the side of the root at the depth h' below the lower mean level. And suppose, as usual, that

$$h' = \frac{\rho}{\sigma - \rho} h.$$

Neglecting the thickness of the crust, which is immaterial to the demonstration, the tangents at these two points will meet at the same point in the crust. This follows because h' bears a constant ratio to h. Let l be the distance from the common ordinate $h + h'$, where the tangents meet. Then, resolving the forces along the two tangents, we have,

$$\frac{\text{Force below}}{\text{Force above}} = \frac{(\sigma - \rho)\, g \sin\theta'}{g \sin\theta},$$

$$= \rho \frac{\sqrt{l^2 + h^2}}{\sqrt{l^2 + h^2 \dfrac{\rho^2}{(\sigma - \rho)^2}}}.$$

Consequently the

$$\text{force below} > = < \text{force above},$$

$$\text{as } \frac{l}{h} > = < \left\{\frac{\rho^2\{1-(\sigma-\rho)^2\}}{(\rho^2-1)(\sigma-\rho)^2}\right\}^{\frac{1}{2}};$$

or, with the usually assumed values of ρ and σ,

as $$\frac{l}{h} > = < 3{\cdot}7.$$

But $$\frac{l}{h} = \cot\theta;$$

$$\text{and } 3{\cdot}7 = \cot 15^\circ \text{ nearly};$$

and $$\cot\theta > \cot 15^\circ \text{ if } \theta < 15^\circ.$$

Hence, when the angle of inclination of the mountain is less than 15°, the force beneath, which urges the melted rocks along the surface of the root, will be greater than that which would urge a liquid along the surface of the mountain vertically above it. Hence, by comparing it with the behaviour of subaerial lava, we can see that the tendency for the rock fused off the root to spread itself beneath the crust will be considerable. We shall therefore have beneath the bottom of the crust areas where the matter fused off the roots of the mountains, and more or less mingled with the denser substratum, is spread out. It is probable that these areas will be local, and not extend beyond a certain distance from the roots of the ranges. They will be analogous to the alluvial deposits, which cover the plains of the upper world. When an eruption occurs above any of these areas, this fused matter will be the first to be ejected, mingled with water-substance already imparted to it by diffusion from the magma beneath it; and, if this fused matter is supplied sufficiently rapidly, it will be the only kind of rock ejected, except such as may be derived from the sides of the funnel. Having been derived from the material of the roots, and in its flow mixed with the denser magma in various proportions, the resulting lava will conform to the law of Bunsen, which asserts that all eruptive rocks are compounded of

two normal magmas, an acid and a basic, mingled in definite proportions. Such will be the case with our lava, if the roots of the mountains have been formed out of a crust consisting in its lower parts of granitic rock beyond reach of the region penetrated by sedimentary deposits. But when we remember that all sedimentary deposits have been ultimately derived from these two normal magmas, it seems that, if samples of every sedimentary deposit proportional in mass to the whole deposit, were mingled together, we should obtain a mixture chemically equivalent to a mixture of the magmas. Accordingly, if many varieties of sedimentary rocks were to contribute their share to the fused matter, it might be difficult to say that the resulting rock had not been directly derived from the two magmas. It does not appear requisite therefore that, because a volcanic product conforms to the law of Bunsen, it should be necessary to exclude the presence of such small contributions as might be expected to be made by sedimentary rocks, depressed here and there into the region of fusion, as would happen if the views of Prof. James Hall and other American geologists are correct[1].

It is now possible to account for the changes in the character of the lava which is emitted from the same volcano at different times. For the fused matter will be distributed in layers, that which is nearest to the bottom of the crust being the most similar to it in composition. It might happen that all this fused matter within reach of the vent might be erupted, and before it accumulated afresh, the lava of the next eruption might be supplied by a denser couche beneath. But before a third took place, the fused matter might have collected again, and possibly this time mingled with the substratum in a different proportion. Thus successive eruptions would produce different lavas. The volcanoes more distant from mountain ranges might be expected, on the whole, to erupt the more basic lavas. It would be worth while to inquire whether there is any reason to suppose this generally to be the case; for we gather from Richthofen, that certain basaltic fissure eruptions

[1] p. 201.

have taken place at a greater distance from the mountain range, than those of a more acid character[1].

The "Natural System of Volcanic Rocks," established by Richthofen and confirmed by Captain Dutton[2] and other geologists, appears to be explicable upon the principles we have been now advocating. Richthofen defines the volcanic era, as that which came in towards the close of the Eocene period, and extends to the present day. This period includes the epoch of the chief elevatory efforts, which have affected the greater mountain chains of both hemispheres. He classes the eruptive rocks of this era under five orders, which are

(1) Rhyolite iv,
(2) Trachyte iii,
(3) Propylite i,
(4) Andesite ii,
(5) Basalt v.

These are arranged in order of the proportion of silica which they contain. But their order of successive eruption in regard to time is different, and is

(i) Propylite 3,
(ii) Andesite 4,
(iii) Trachyte 2,
(iv) Rhyolite 1,
(v) Basalt 5.

Thus the first erupted were the rocks intermediate in respect of their acidity and specific gravity, (i) and (ii). These were followed, according to Richthofen, by those of greater acidity and less specific gravity (iii) and (iv); and later by those of least acidity and greatest specific gravity (v). This order of succession appears inexplicable on the simple theory of fluid

[1] Richthofen says that Andesite occurs on the southern slope of the Carpathians, p. 25; and that Basalt occurs in the neighbourhood of the more ancient volcanic rocks (among which he classes Andesite), accompanying their ranges at some distance, itself forming extensive ranges: p. 27. Putting these statements together, it appears that the basaltic range is distant from the mountains.

[2] "Geology of the High Plateaus of Utah," Chaps. IV., V.

couches, superimposed in reverse order of their specific gravities.

The manner in which Richthofen explains this succession is satisfactory as regards the first and last, but not equally so as regards the intermediate rocks. Premising that the eruptive rocks of palæozoic times were chiefly of an acid character, and that little eruptive activity was manifested during the mesozoic, he proceeds[1], "Those siliceous compounds especially, of low specific gravity, which had formerly yielded the material of the vast accumulations of quartziferous eruptive rocks, would have been consolidated, and the limit as it were between the solid and the viscous state of aggregation receded into regions where the matter would be of a less siliceous composition and of greater specific gravity." * * * When the eruptive activity was renewed, "The first rocks ejected would necessarily be of a more basic composition than the predominant rocks of the granitic era, while the repetition, at a later epoch, of the process of fracturing would give rise to the ejection of rocks in which silica would be contained in still lower proportion. The greater portion indeed of the ejected rocks consisted of propylite and andesite, in the first, and of basalt in the second half of the volcanic era." Thus far all is plain. But when he offers an explanation of the intermediate ejection of a more acid and lighter rock than any of these his reasoning is less convincing. "A notable but only apparent anomaly in the regular order of succession has been the emission of trachyte and rhyolite, between the andesitic and basaltic epochs. But if it is considered that these rocks were ejected partly from the same fractures through which andesite had ascended, and partly from others in their immediate vicinity, while the distribution of basalt has been independent, to a certain extent, of all foregoing eruptions, it is evident that the occurrence of trachyte and rhyolite is closely dependent on that of andesite and bears only a very remote relation to basalt. It appears that after the ejection of the chief bulk of andesite, when other processes ending in the opening of fractures into the basaltic region were being slowly prepared in depth, the seat of eruptive

[1] "Natural system of Volcanic Rocks," p. 58.

activity ascended gradually to regions at less distance from the surface. There is within the limits of conjecture based on physical laws, no lack of processes which could co-operate to that effect. The consolidation of the ejected masses within the fissures would probably proceed simultaneously, by loss of heat, from the surface downwards and, by pressure, from below upwards. The opening of new branches from the main fractures, the remelting (by the aid of the heat of the molten mass within the latter, and of water finding access to it) of solidified matter adjoining the fracture, the emission of that remelted matter through those branches: all these are secondary processes depending on the first almost necessarily. The supposition that to those is due the order of time in which trachyte and rhyolite have been ejected to the surface, is corroborated by the fact that these rocks occupy generally a subordinate position in regard to quantity and have had to a great extent their origin in volcanic action"—that is, as distinguished from massive or fissure eruption. It is clear that, in order to obtain the acid magma required for the production of the eruptions of trachytic rocks subsequently to the andesitic, some method must be devised to explain the refusion of the granitic crust, lying, already consolidated, above the andesitic magma. Richthofen proposes to do this by "the aid of the heat of the molten mass" within the main fracture, "and of water gaining access to it." This appeals to the very doubtful hypothesis already referred to, which supposes "glowing lava," not already liquid, to absorb water admitted to it from without, and thereby to become liquid and light. Moreover it requires the "molten mass within" the fissure to contain an excess of heat sufficient to melt an additional mass of a more refractory rock than itself. But it is evident that it could not transfer more than so much of its own heat to another mass, as would raise the two to a common temperature, less than its former temperature.

Captain Dutton's discussion of the same sequence is interesting and instructive, and well worthy of study, but still not satisfactory. The general principles employed by him are indicated in the following sentence: "In order that any eruption of lava may take place two preliminary conditions are requisite. First,

The rocks must be fused. Second, The density of the lavas must be less than that of the overlying rocks. Having shown from independent considerations that the proximate cause of volcanic activity may be a local rise of temperature in deeply seated rocks, it only remains to follow the obvious phases of the process." He then founds, upon a previous discussion of the respective temperatures at which the different rocks would become at once fusible and sufficiently light to reach the surface, an argument to show that Richthofen's order of succession agrees with the order so arrived at. "It is just possible that the acid rocks may be light enough to erupt at an early stage of the process"—of a local rise of temperature—"but are not yet melted, and that the basic rocks may be melted, but must await a further expansion in order to reach the surface. The first selection would then fall upon some intermediate rock.[1]" Several objections to this explanation are mentioned in a short critique upon this, in general admirable, Work by Prof. A. Geikie[2]. But the fundamental objection already brought against it, of the impossibility of explaining the "local rise of temperature" postulated, seems to us fatal[3].

It appears however to Prof. Judd that the exceptions to Richthofen's law as precisely stated, "are so numerous as to entirely destroy its value. The generalisation that in most volcanic districts the first ejected lavas belong to the intermediate group of the andesites and trachytes, and that subsequently the acid rhyolites and the basic basalts made their appearance, is one that appears to admit of no doubt, and is found to hold good in nearly all volcanic regions of the globe which have been attentively studied"[4]. But even this generalisation is sufficient to necessitate some explanation. Prof. Judd says also that "it lends not a little support to the view that under each volcanic district a reservoir of more or less completely molten rock exists, and that in these reservoirs various changes take place during the long periods of igneous activity. During the earlier period

[1] "Geology of the High Plateaus of Utah," p. 131.
[2] "Nature," XXII. p. 324. 1880.
[3] p. 241.
[4] "Volcanoes," p. 200.

of eruption the heavier and lighter elements of the contents of these subterraneous reservoirs appear mingled together; but in the later stages of the volcanic history of the district, the lighter or acid elements rise to the top and the heavier or basic sink to the bottom, and we have separate eruptions of rhyolite and basalt." But he gives no reason for this original mixture of the magmas and for their subsequent separation.

According to our view, however, the succession of Richthofen seems to be the necessary one. Before the rending of the fissures occurred, which we suppose to have elevated the mountain chain, the different magmas occupied the positions and sequence determined by their densities, and consequently the acid magma being already solidified in the crust, the first erupted of the rocks would be intermediate in respect of their specific gravity, and acidity. But this very injective action itself would depress the acid crust in a protuberant ridge, presenting to the denser substratum an under surface, which would be but just below the temperature of its fusion. This surface now becoming fused, would underflow the crust, and the next time that an eruption took place in that region, a more acid and lighter lava would make its appearance. But at other places, out of the reach of the flow of this acid remelted matter, or at the same place after the layer of it thus formed had been ejected, the subjacent basic magma would furnish the lava next in sequence.

The same hypothesis appears capable of accounting for such a circumstance, as that mentioned by Prof. Judd regarding the adjacent volcanic districts of Hungary and Bohemia[1]. The products of the contemporaneous later tertiary outbursts in these adjacent areas were "as different in character as can well be imagined," so that "it is scarcely possible to imagine that such very different classes of lavas could have been poured out from vents which were in communication with the same reservoirs of igneous rock, and we are driven to conclude that the Hungarian and Bohemian volcanoes were supplied from different sources." According to our view they were so; because the areas which

[1] *Ibid.* p. 202.

supplied these lavas were separated by the roots of the Carpathian mountains, and we should no more expect exactly similar flows of rock on opposite sides of the inverted mountains below, than we should expect similar alluvial deposits over the two subaerial areas separated by their watershed. Indeed the difference might be even greater, because a mere difference in the steepness of the root on the two flanks, by its effect on the friction between the two, would cause the melted matter and the substratum to be mingled in different proportions. Moreover the substratum itself, owing to the disturbance caused by eruptions, may have some amount of circulation set up in it, and since it goes to make up a considerable portion of the erupted matter, this, if it occur, would cause the lavas to be different.

It must be borne in mind that we have not been engaged in inventing a theory, to account for the natural sequence of volcanic rocks; for we have, as we believe, already established the existence of our mountain roots, and have been doing no more than to show how they serve to explain the succession in the order of eruption.

CHAPTER XXI.

GEOGRAPHICAL DISTRIBUTION OF VOLCANOS.

The Linear arrangement of Volcanos accords with the doctrine of a thin crust and fluid substratum—Volcanic bands related to the boundaries of continents—Distinction between coastline and oceanic volcanos—Darwin on coral islands—Platforms on which oceanic islands stand—their possible origin—Great volcanic band of the Pacific coast—Sinking and rising areas adjoining it—It follows nearly a great circle of the sphere—and divides the land from the water-hemisphere—Is not equally active at every part—Suggested cause of local activity—Other possible causes of the same—Unknown cosmical causes have operated—Conclusion.

THE Geographical distribution of volcanos presents perhaps fewer difficulties upon the supposition of a thin crust and a fluid substratum, than upon any other that can be made, regarding the constitution of the outer parts of the globe. The linear arrangement of the greater number of the vents, points to their situation along systems of fissures, and represents on a grand scale the same phenomenon, which occurs, when subsidiary cones of eruption are established upon fissures radiating from a central volcano. To explain why these fissures, extending for thousands of miles, should range in certain directions rather than in others, is probably a hopeless task. The causes which contribute to determine the direction of any fissure are very complicated, and in the case before us generally unknown; and the positions of those which underlie the at present active vents, are not the same as of those which did so in more ancient times. The post-

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eocene volcanic bands are no doubt nearly related to the recent ones, but those of previous periods have probably little connection with any now existing.

The great volcanic bands of the present day have an obvious relation to the boundaries, which separate the oceans from the continents. They skirt the coast-lines, either upon the edge of a continent, as along Western America, or else they occupy chains of islands parallel to the continental shore, as is the case along the Eastern coast of Asia.

Besides these coast-line vents, others are found in the open ocean. These also appear to hold still a certain relation, though an altogether different one, to the coast-lines. Their position is medial. Thus in the Atlantic they occupy a medial line, rudely parallel to the opposite coasts; and in the Pacific, the boundary of which is somewhat circular, they are found in a central patch in the Hawaian group.

It also appears that an important distinction may be drawn between the modes of occurrence of coast-line and of oceanic volcanos. The cones raised by the former in some instances, no doubt, emulate in height, if they do not overtop, the crests of the ranges upon which they are parasitic. But they can by no means be considered as constituting a largely integral part of any mountain chain. If then such a chain were to be submerged, until only the tops of the highest mountains were left uncovered, the resulting islands would not as a rule consist of volcanic products, but, on the other hand, they would chiefly present crystalline or schistose rocks. Now the case is exactly the opposite with oceanic islands. They are all volcanic. Darwin, in his work on "Coral Reefs," tells us that "the geological nature of the islands which are encircled by barrier reefs varies; in most cases it is of ancient volcanic origin; owing apparently to the fact that islands of this nature are the most frequent within all great seas; some however, are of madreporitic limestone, and others of primary formations, of which latter kind New Caledonia offers the best example[1]." So also are some of

[1] "On the Structure and Distribution of Coral reefs." Second Ed. 1874, p. 62.

the Comoro Islands, and the Seychelles[1]. Now madreporitic rock, having probably grown upon a volcanic basis, need form no exception to the general law. And a glance at the map will show that neither New Caledonia, nor the Comoro and Seychelles Islands, are properly speaking Oceanic; the former belonging to the submerged ridge, which connects New Zealand with Australia and South-Eastern Asia, and the latter being a continuation of the axis of the great Island of Madagascar. The oceanic islands of the Atlantic are likewise volcanic. There is then it seems the important distinction to be drawn between coast-line and oceanic volcanos, that the former are connected with axes of elevation, and the latter not so.

At the same time it is known that the volcanic oceanic islands rise from platforms elevated above the general level of the ocean floor. Still, if these were formed by compression, we ought to find some of the islands presenting exposures of the inclined strata of their higher ridges, which is not the case. Of what then do these platforms consist? Is it not possible that they may be accumulations of the ejectamenta of the volcanos themselves? With the fact before us that 665 feet of volcanic ash and tufa were pierced in sinking an Artesian well at Naples[2], we may well believe that the enormous volcanos, whose ruins now remain in the shape of Oceanic islands, may have laid down an immense thickness of deposits around their bases.

If this be a true account of the condition of these areas, the agency required for their production is one of fissuring without resulting elevation. To account for this we can but speculate. It may be that the suboceanic crust is too rigid to yield to the compressing force, which, as will be remembered, is not excessive; for the doubt which we have felt is whether it is sufficient for the elevation of continental ranges. Or it may be that the fissures beneath the ocean do not stop short until they reach the surface, and so the compressing force has not time to act; for, as we have seen, when a fissure is completed to the surface compression ceases. Another suggestion to be offered is that,

[1] *Ibid.* p. 69.
[2] "Comptes Rendus," tome XLVIII. p. 994. 1859.

if as believed the suboceanic crust is not much less dense than the substratum, even if there were compression, the resulting elevation would be comparatively small, because much more of the compressed matter would go to form the roots, than would be the case with a lighter crust.

There is a distinction to be observed between an erupted cone and a mountain produced by compression. The process of formation of the latter causes a downward protuberance, which assists by floatation to support the weight. But no such protuberance accompanies the piling up of a volcanic cone. Consequently, where that is formed distant from any mountain chain, the tendency to rupture, and sink through the crust, will be uncompensated. In the oceanic areas the downward pressure of the cone will of course be lessened by the weight of the water displaced by it; but on the other hand the difference of specific gravity of the crust and the substratum being, as we believe, less than in continental areas[1], the support furnished from below will be less. The result will be that an oceanic volcanic area will have a tendency to perpetuate its own existence by fissuring the crust around its margin, and along the fissures so formed fresh eruptions might be expected to break forth. The elliptical form of many coralline archipelagos may perhaps lend countenance to such a supposition, for a train of volcanos, established originally upon a single line of fissure, would develop a figure of that form during the process we have suggested.

Reverting to the trains of volcanos which range parallel to the coasts of the great continents, we observe that, along the boundary of the Pacific Ocean, where near the American shore deep water is found not far from land, there the volcanos stand on the edge of the continent. But where, as in the Aleutian Islands, Japan, and the more southern parts of the western area of the Pacific, no great coast-range of mountains exists, and deep water is not found near the continent, there is found a great fall in the bed of the ocean on the outer, or Eastern, side of the Islands which carry the volcanos. Here then, if the

[1] p. 165.

water were removed, the same fact would be apparent, that the volcanos occupy an elevated tract, which borders the great continent. Such an arrangement of the vents, regarded as the indication of systems of fissures below, of which probably a few only reach the surface, is in accordance with our theory, that the same ultimate cause underlies the phenomena both of compression and of volcanic activity.

Richthofen has made the remark that "active volcanoes have been found to be particularly numerous in those regions where the narrow terminations of two continents verge towards connection, as in the case of Central America, between Alasca and Kamtschatka, and between Australia and Farther India[1]." The crust movements of the last-named region, and of the surrounding oceans, have been made more familiar to us than those of most other parts of the world, through the researches of Dr Darwin on Coral Reefs. He has shown that the areas occupied by Atolls and Barrier reefs are sinking areas; while those in which active volcanos prevail are rising; as testified by the occurrence of beds of recent shells at considerable heights above the sea level, and corroborated by the fringing reefs without lagoon-channels, that encircle the islands. The chart prefixed to his work gives at a glance the relative positions of the sinking and rising areas within the coralline seas; and we perceive that the trains of volcanos, which are the most active in the world, are of the coast-line type, and that they follow the axial trend of the land masses. The rising areas are confined to comparatively narrow bands, bordered by the broad areas that are sinking. This arrangement of the masses agrees well enough with our section of a disturbed tract[2], in which we see that, if compression were to affect the elevated ridge *A*, *A* would become part of a rising area, whilst wide regions at *b*, *b*, on either side of it would sink.

We may, perhaps, conclude that the continent of Australia is at present in process of becoming united to Asia, as we are led by geological reasons to believe that North and South America have been comparatively recently joined.

[1] "Natural System of volcanic rocks," p. 79.

[2] p. 132.

We are not obliged to suppose that those oceanic areas, now shown by Atolls and Barrier reefs, attached respectively to sunken or existing islands, to be undergoing depression, were ever continental areas, because, as already mentioned, these islands are not composed of elevated strata, but, where visible, consist of volcanic rock of a former age, a type of island belonging then, as now, to the open ocean.

The great Pacific train of volcanos divides the world very nearly into two halves, as may be seen by placing an ordinary globe so that the wooden horizon passes over the Isthmus of Panama, the southern extremity of Kamtschatka, and the straits of Sunda. Near the latter place, however, the train becomes more complicated; for it takes a turn to the east at the Philippines, and is joined by that other train which passes through Java, New Guinea being at the place of junction, and the great Island of Borneo occupying the angle. Afterwards the combined train turns round towards the south, till it reaches New Zealand, following a course nearly parallel to the eastern coast of Australia.

If we look at the globe thus placed, we observe that we have nearly all the land on one side of this great train, and that the other hemisphere, except Australia, is occupied by water. It appears then that, with this exception, the said volcanic band defines the elevated part of the surface. But the exception proves the rule. For, when it nears Australia, the direction of the main band is diverted by its junction with the second, and within the great curve which their united course follows, this insular continent is embraced.

Regarding the great Pacific volcanic band under this aspect, it is seen to follow nearly a great circle of the sphere for more than half its circumference, when it is disturbed by encountering a second, which diverts it from its direct course. It can hardly be that any cause, less than one of a very general character, affecting the spheroid as a whole, can have produced this unique, so to speak, nearly straight fissure. We are disposed to attribute its origination to some unknown cosmical cause.

But this great band is not now equally active at every part. In seeking the conditions which produce those disturbances of the crust that determine the regions of present activity, by admitting at intervals of time the elastic fluids from below, we may look to changes in the distributions of load upon the surface, as has been more particularly discussed in the tenth Chapter. It has been there shown, that the less steeply inclined side of the elevated range will be that on which the principal sedimentation will go on, and, acting as it were on the longer arm of a lever, whose fulcrum is at the centre of gravity of the tract, it will tend to rupture the crust on the margin of the steeper side of the ridge, at the same time causing a certain hang of the tract towards the region of deposit, which will open rather than nip the crust at the place of fracture. We may possibly trace this connection between the situation of the active regions of the Western American volcanos, and the great rivers of that continent, which deposit their sediment on the eastern side. For instance, in Central America, we find a comparatively narrow tract, the western side of which is mountainous, while on the Eastern the Mississippi deposits vast quantities of sediment. The region of its deposit is known to be a sinking area, and must tend to give a tilting movement to the tract so disturbed, raising and fissuring it along its western border, near which its centre of gravity lies. The very same sedimentation may also be the cause of disturbance, where the West Indian volcanos occur.

The wide plain of the Amazon is also an area of great sedimentation, and may have an effect, similar to that just described, upon the coast line adjoining the Andes of Quito. In like manner the deposits from the Rio de la Plata may have a connection with the volcanic series of Chili.

Accumulations of snow and ice would have a like effect, and the subsidence of Greenland and the volcanos of Iceland may be possibly due to the accumulation of snow upon the former. These are however speculations which are merely put forward as suggestions.

There are other causes which, if the crust be no thicker

than we have concluded it to be, may not be without effect upon its equilibrium, such are the action of ocean currents and winds. Slight as is the momentum of a mass of water, moving with the velocity of an ocean current, as compared with the mass of crust upon which it may act, nevertheless its effect cannot be nil. And the same may be said of winds, which, on the average of the year, have definite directions over a given tract of continent, and their friction upon the surface cannot be quite inappreciable. These two causes cooperating, may have had some share in producing the peculiar form of the land masses of the globe. These are however questions which do not come within the scope of the present work.

In bringing this volume to a conclusion, the reader is reminded that it deals with a great number of questions, each in a very superficial manner, and that indeed entire treatises might be written upon the subjects of some short chapters. The whole, with the exception of the attempt to account for the presence of water substance in the fluid magma, has been treated in a manner that can hardly offend the most strict disciple of the uniformitarian school.

But there are phenomena which appear to require for their production causes hitherto unexplained, which must be looked for possibly outside the globe itself. Such are climatal changes. Such is also the remarkable fact that the grand efforts of elevatory action appear to have been paroxysmal, and after slumbering for long ages, to have been more intense at certain periods; the last of which was subsequent to the Eocene. These questions are left for a rising generation of physicists and astronomers, who it is hoped may think the ascertained facts of Geology a worthy field for the application of exact scientific methods.

CHAPTER XXII.

SUMMARY.

[*The Roman numerals refer to the Chapters.*]

I. *On underground temperature*—II. *Condition of interior*—III. *Internal densities and pressures*—IV. *Lateral pressure, its amount*—V. *Elevations and depressions*—VI. *Elevations on the hypothesis of solidity*—VII. *Hypothesis of solidity fails*—VIII. *Fluid substratum*—IX. *Crust not flexible*—X. *Disturbed tract*—XI. *The revelations of the plumbline*—XII. *The revelations of the thermometer*—XIII. *Amount of compression*—XIV. *Extravasation of water will not account for compression*—XV. *Compression and volcanic action*—XVI. *On Faulting*—XVII. *Geological movements explained*—XVIII. *Mr Mallet's theory of volcanic energy*—XIX. *The volcano in eruption*—XX. *Sequence of volcanic rocks*—XXI. *Geographical distribution of volcanos.*

I. We have commenced our discussion of the wide subject of the Physics of the Earth's Crust with underground temperature, because the distribution of heat in the interior of the earth is one of the cardinal conditions upon which all physical questions connected with it depend. We have pointed out that, having regard to such depths as artificial excavations reach, the law of increase is on the whole an equable one, amounting on an average to about one degree Fahrenheit for every 51 degrees of descent. It has been asserted that the rate has been found to decrease towards the bottom of some deep boreholes, and it has been even maintained that at a depth of about 5000 feet the increase of temperature would come to an end. But we have shown reasons for believing, that the observations, in which a diminution in the rate has been said to have been noticed, have really been vitiated by the disturbance of the column of water in the borehole by convection currents, and that the supposed cessation of the increase at about 5000 feet has arisen from a delusive method of computing the general result. Nevertheless it is unquestionable that this equable law of increase of temperature, though probably true near the surface of the earth, cannot extend to all depths; for, if it did, we should

have, at the depth of from 200 to 400 miles, temperatures which would equal those, which have been on good authority attributed to the sun himself. The equable law of increase, however, may be so far depended on, as to lead us to expect a temperature at which the rocks would melt at a depth of not more than 30 miles.

II. We are next led to speculate about the condition of the earth's interior. And first we enquire what is the law of density? The density of the surface rocks can be fairly estimated by actual observation in more ways than one, and is found to lie between 2·56 and 2·75 times that of water. The mean density of the whole earth has been determined by experiments with the plumbline, and torsion balance, and may be taken to be about 5·5. There is little doubt that the interior of the earth is in a sense stratified, and consists of layers, or *couches de niveau*, each of equable density, and that the density is greater towards the centre, and diminishes towards the surface. But it is not known for certain what the actual law of density is; that is to say, we do not know how great the density is at any given depth, nor how thick any stratum is, which may be supposed to have a particular density. All that we really know is that the heavier layers are deeper down than the lighter, and that the forms of them are spheroidal, becoming more and more flattened about their poles as they approach the outer surface.

Now these are the arrangements and forms which the strata of a liquid spheroid would assume under the action of rotation and gravitation. It has been consequently concluded that the earth was once wholly melted.

Here, then, two lines of argument meet. The present law of temperature is such that we may expect to find heat enough at the present day to melt the materials of the earth at the depth of about 30 miles, and mathematical investigations, concerning the figure of the earth and its law of density, lead to the conclusion that it has been melted. The question arises, whether it is at this day molten within, either wholly or partially, or has the molten condition passed away, so that it is now solid? There is however a *tertium quid*. Some have

thought that it is what has been called 'potentially fluid,' that is, wholly solid in consequence of the pressure to which the internal strata are subjected, but ready to become fluid, on account of its high temperature, whenever the pressure is relieved. This is the great subject of controversy, towards the determination of which the present volume is intended as a contribution.

The increase of temperature below the outer crust, which we know to be solid, will be governed by the laws of convection of heat if the interior is fluid, but by the laws of conduction if it likewise be solid.

The question whether the interior of the earth is at present solid or fluid, or partly solid and partly fluid, may, apart from geological considerations, be attacked in two ways. The first of these is by enquiring, what is the difference of effect that the moon and sun would have upon the motions of the earth in these cases; and the other way is by considering the sequence of events, according to which a molten globe may have passed into its present state. For the first, there are two kinds of effect produced by the attraction of the moon and sun upon the earth; and these are precession and the tides. Mr Hopkins, between 1839 and 1842, investigated the difference between the precessional effects in the cases of a solid and of a fluid interior, and came to the conclusion that the crust of the earth could not be less than from 800 to 1000 miles thick. But the arguments of Mr Hopkins were, in 1876, abandoned by Sir William Thomson.

With respect to the tides the case is different. The last-named eminent physicist says that unless the earth as a whole were extremely rigid, "the solid crust would yield so freely to the deforming influence of the sun and moon, that it would simply carry the water of the ocean up and down with it, and there would be no sensible tidal rise and fall of the water relatively to land." It does not however appear necessary that the earth should be absolutely solid from the centre to the surface to satisfy the requirements of great rigidity as a whole. May this not be satisfied by the hypothesis of a rigid nucleus, nearly approaching the size of the whole globe, covered by a

fluid substratum of no great thickness compared to the radius, upon which a crust of lesser density floats in a state of equilibrium? This is the supposition as to the condition of the earth which appears on the whole to satisfy best the requirements both of geology and of physics, as we have attempted to show in the preceding chapters.

That the above arrangement of the materials of the earth is compatible with the laws of cooling of such a body, has been proved by Mr Hopkins to be the case. The nucleus owes its solidity to the enormous pressure of the superincumbent matter while the crust owes its solidity to having become cool. The fluid substratum beneath it is not under sufficient pressure to be rendered solid, and is sufficiently hot to be fluid. As to the degree of its fluidity, we have no certain knowledge. It is probably more viscous in its lower portions through pressure, and likewise passes into a viscous state in its upper parts through cooling, until it joins the crust. But how fluid the most fluid intermediate parts may be we cannot determine.

III. It has been stated that the mean density of the whole earth is known, and also the density of surface rock. But the actual densities of the successive layers, and their corresponding forms and thicknesses, are not known. There must be certain relations among these attributes in order to satisfy the ascertained facts regarding the earth's shape, and its mean and surface densities. But these requirements may be satisfied in more ways than one. Laplace suggested a law of density, suitable for purposes of calculation, which satisfies these requirements very closely; and that law has been generally assumed, as representing the true state of the case, in investigations on the figure of the earth. It makes no assumption as to the actual cause of the variation of density with the depth, although it is assumed to be such as might be caused by a certain relation between density and pressure. But it is much more likely that the increase in density towards the centre of the earth arises from the heavier materials gravitating thither. Waltershausen has formed a theory on the ground that the earth is a hot globe, of which a considerable portion is fluid,

an unknown amount of the central parts being rendered solid by pressure. The downward increment of density is expressed by the chemical increment of the heavy bases: and the fluid region directly under the crust consists, first of a felspathic and acid magma, which passes downwards by successive replacement of bases into an augitic and finally, into a magnetitic magma. This theory appears to have a great deal to recommend it. Waltershausen has given the probable densities and pressures at different depths, but the formula by which he has calculated them appears to be arbitrarily assumed. He makes the density at the centre of the earth 9·59, or about that of silver, and the pressure there that of about 2,500,000 atmospheres. If, however, we use Laplace's law of density, the pressure at the centre comes out considerably greater, reaching that of 3,000,000 atmospheres.

There will be found given in the appendix a new theory of the constitution of the interior of the earth by M. Roche, which will satisfy the requirements of geology, provided the difficulty, connected with the change of rotational velocity, which we have suggested, can be satisfactorily met.

IV. Having devoted the first three chapters to the consideration of the more general questions regarding the constitution of the earth as a whole, we now approach the geological aspect of the subject. The outer crust of the earth is the peculiar domain of geological enquiry. And enquiry leads to the conclusion that this crust has been subjected to great violence, being compressed, ruptured, raised, and depressed, and also subjected to heating. A special phenomenon of a most startling kind, did not frequency of description render us familiar with it, is that in certain districts, outbursts of fiery vapours and molten rock, accompanied by violent earthquakes, mark the volcanic regions of the surface. To account for both these classes of phenomena a source of energy is required, and to the earth's interior we must look to supply it. Here we find gravitation, and heat stored up ready for our purpose. How have these agents produced the results we witness? This is our problem.

The well-known fact, that great lateral compression has affected the stratified rocks of the earth's crust, is now generally explained by the supposition that the globe has contracted through secular cooling. It is thought that, as the cooling proceeded, the interior shrank away from the crust, and the latter became wrinkled; and that by this means the crumpling and contortions of the rocks were produced. We have accordingly calculated what the lateral pressure would be, which would be available for crushing the strata of the earth's surface, supposing that the interior were to shrink away from the crust, and to leave it unsupported. We find that it amounts to the enormous pressure of the weight of a column of rock of the surface density, of the same section as the stratum, and *two thousand* miles long, or about 830,200 tons upon the square foot. We need not doubt that this pressure would be competent to perform the work expected of it.

Nor would any solid stratum in the interior of the earth be capable of sustaining the lateral pressure upon it: for these lateral pressures would be still greater within the earth than at the surface, except very near the centre.

V. That the pressure thus produced would be abundantly sufficient for the purpose, is however no proof that the work has been accomplished in that way. It has been an assumption often repeated, but never proved. The first task which we have proposed to ourselves is therefore to examine this point. We admit that the inequalities of the earth's surface have been caused by lateral compression, but we are not sure that this has arisen from the secular cooling. We therefore commence our enquiry by seeking for some measure of the inequalities of the surface, as a preliminary step towards determining how they have been produced: and in the first instance we include the greater inequalities, which constitute the oceanic and continental areas. But although we have ocular proof that mountain chains have been formed by compression, it is mere matter of inference that the elevation of continents above the ocean floor, is likewise due to the same cause.

Suppose then the earth to have contracted through cooling,

and suppose (which is of course impossible) that the crust had shrunk down upon it without becoming either thickened or wrinkled by the process. The position which the surface would have occupied under this impossible supposition, gives us a definite level, at a definite distance from the centre of the earth, and this level we call the 'upper datum level.' Similarly, the position which the under side of the crust would occupy, we call the 'lower datum level.' To these levels we refer the elevations and depressions (if any) of the surface, caused by the crumpling action; and we can express their amount by saying how deep a layer of material the elevations would form, if they were levelled down, and spread out upon our upper datum level.

Now if we assume that the earth is solid, *i.e.* that there is no fluid substratum beneath the crust, it is clear that all the inequalities which compression could produce would be of the nature of elevations above the upper datum level. There could be no depressions. Moreover the bottom of the ocean basins would either coincide with this datum level, or else they would be the parts of the disturbed surface least raised above it.

Assuming the former to be the case, we find, upon an estimate as near as we see our way to form, that the whole of the existing inequalities of the surface, if levelled down and spread out, would form a layer of from 9500 to 13000 feet thick over the whole earth. This estimate is not likely to be too large, but rather the other way.

VI. Our next step is to enquire, what the amount of the inequalities would be, if they had been formed by lateral compression through cooling. If we can make an estimate of these, and compare it with that which we have already made of the existing inequalities, we shall be able to form an opinion as to whether the cause assigned is, or is not, adequate for the purpose.

Now it is fortunate that Sir William Thomson has already paved the way for such an investigation, by establishing the law which the temperature within the earth must follow, if it be a solid cooling by conduction. We are consequently able to

say how much it would have cooled at any specified depth from the surface, since the whole became solid. If then we know the amount of contraction on cooling for such rocks as exist near the earth's surface, we shall have the means of calculating how great the contraction would have been; and consequently what amount of inequalities this cause could have produced. Again, Mr Mallet has made a series of careful experiments on a large scale, on the contraction through cooling of silicious slags from a state of fusion, and his results are sufficiently applicable to our problem. These we have used, and calculated the amount of inequalities of the earth's surface, which would have been formed had they been due to a hot solid globe cooling by conduction, and we have found the amount to be very far smaller than that which actually exists. For whereas, as we have seen, the actual inequalities, if levelled down, would form a layer of about 10,000 feet thickness, those which would be formed on the present hypothesis would form a layer not exceeding 900 feet in thickness on the most extravagant supposition. But if we adopt a more moderate basis of computation, 200 feet would be nearer the mark.

VII. The large discrepancy between these quantities shows that the hypothesis, that the inequalities of the surface have been caused by the cooling, and consequent contraction of the earth regarded as a solid globe, is untenable. We appear then to be compelled to accept one or both of the alternatives, (1) either the inequalities of the surface are not altogether, or even chiefly, due to lateral compression, or (2) there has been some other cause involved in producing the inequalities of the crust, other than the contraction of a solid globe through mere cooling.

In the estimate we have made of the inequalities of the surface, which might arise through compression from the cooling of a solid globe, we have included the basins of the oceans. If these be due at all to the contraction of the materials of the globe, there does not appear to be any other way of accounting for them except by compression: because, if they had been caused by direct radial contraction, they would require a differ-

ence of linear contraction equal to their depth in the matter beneath themselves and beneath the land. But in the course of our calculations it has appeared that the entire radial contraction would not be equal to the depression of the ocean bed beneath the land surface. We have then sufficiently proved, that the contraction of a solid earth through mere cooling is not adequate to account for the inequalities of the surface; and it also appears probable that the other alternative must be accepted likewise, because the ocean basins being regarded as inequalities, or depressions, of the surface, they cannot be directly accounted for by compression; either as depressing them, or relatively raising the continents. Still further, the distribution of land and water upon the globe, as Herschel has observed, proves the force by which the continents are sustained to be one of tumefaction, and is therefore a proof of the comparative *lightness* of the materials of the terrestrial hemisphere; so that an excess of density appears to be requisite to retain the water over such extensive areas as are occupied by the greater oceans.

Well-known geological phenomena, which prove the instability of the earth's surface, also negative the hypothesis of solidity. Alternate elevation and depression have frequently affected the same areas. But especially the shifting of the crust towards a mountain-range, which is testified by the corrugation of the rocks of which it is formed, requires a more or less fluid substratum to admit of it. The sinking of areas such as deltas, and other regions of deposition, demands a like arrangement; and in short, it appears that the crust, in the form in which it exists, must be in a condition of approximate hydrostatical equilibrium, such that any considerable addition of load will cause any region to sink, or any considerable amount denuded off an area will cause it to rise.

VIII. If we admit that the cooled crust of the earth rests upon a fluid substratum, we cannot doubt that the fluidity is a consequence of its high temperature; and it probably follows that volcanic eruptions arise from emanations from the substratum gaining access to the surface. All volcanic eruptions are

accompanied by a great emission of steam: but it is not agreed whether this water-substance forms an integral part of the substratum, or whether it becomes in some way subsequently mingled with the lava during, or just before, its eruption. The position of volcanos near the sea, and the sea salts given off by the lava, have been held to favour the latter supposition. Water can be conceived to gain access to masses of heated rock only in two ways; by open fissures, or by capillary absorption. We have, we think, shown that neither mode of access is possible. It remains that this water, and the elements of sea salts, must be original constituents of the magma. The question then is, how this water-substance comes to be there. To answer this it is necessary to go back to the cosmogony, and we suppose that, under the pressure due to the water-substance which would have, in a state of vapour or gas, formed the outer layers of the still incandescent earth, this vapour or gas would not have been necessarily superincumbent on an anhydrous globe, but that such rocky matters as were soluble with it would have been in solution with it. As cooling proceeded, an upper crust of rock would have been formed by crystallization at a certain level, and would have gradually extended downwards. But we conceive that there still exists an intensely hot layer of water dissolved silicates, in a state of what has been called igneo-aqueous fusion, underlying the solid crust, ever in readiness to furnish the steam, gases, and ejectamenta, of the volcano.

This igneo-aqueous fusion has usually been spoken of as a state of solution of the rock in water. It is a condition of which we know little. Perhaps it may be more correctly described as a state of solution of water in rock. And some recent experiments by Mr Hannay throw an interesting light upon the subject. He doubts the conclusions of Andrews as to the continuity of the states of matter, and it would appear from his reasoning[1], that, when a substance is confined at a temperature above the critical, it is really in the gaseous state; and that in this state it is capable of holding solids in solution,

[1] *Proc. Roy. Soc.*, Vol. XXXII, p. 410, June 16, 1881. See also *ibid.* p. 407.

which in the vaporous state it cannot do[1]. We may therefore look upon the state of igneo-aqueous solution as one, in which the water-substance is in a gaseous state, and that the combination between the water-substance and the rock is probably of that kind, which has been termed "occlusion" of gas by a liquid.

IX. In considering the form and arrangement of the inequalities which a thin crust might assume, if it rested upon a liquid substratum which had from any cause fallen away from it, so that it became subjected to lateral compression, the supposition may be made that the crust is flexible—that is to say, that it accommodates itself to its new position by bending without breaking, or becoming thickened anywhere. Under these circumstances the fluid would rise into the anticlinals. The form which such a crust would assume has been approximately determined in the ninth chapter, and it has been shown, that there is no reason to think that it accords with the arrangement of the inequalities on the earth's surface. We therefore dismiss this hypothesis.

If the crust does not maintain a uniform thickness, it must accommodate itself to compression by being crushed together, and thickened in places. The very important consequence follows, that elevations above the datum level will be accompanied by depressions beneath. The anticlinals will not be filled with fluid from below, but will be the upper portions of double bulges, which will dip into the fluid below, as well as rise into the air above. From whatever cause compression might arise this result would be the same.

[1] "The definition of the gaseous state as a state of matter not alterable by pressure alone leads us to a clear division of aeriform matter into two states, the vaporous and gaseous, the first alterable, the second unalterable by pressure alone. Another distinction between vapour and gas is this: gases are solvents of solids; vapours are not. Let a liquid be coloured by having some non-volatile coloured solid dissolved in it, and let it be heated under pressure the liquid will remain coloured while the vapour will be quite colourless, and wii so remain up to the critical point. Now let the fluid be raised above its critical point, all the internal space will be coloured, showing that (the contents being gaseous) the gas dissolves the solid while the vapour does not."—*Proc. Royal Soc.* Vol. XXXII. p. 412. This has a bearing upon what has been said about the formation of mineral veins, p. 198.

X. We next attempt to deal with the condition of the earth's crust just described. It will be observed that it is analogous to the case of a broken-up area of ice, refrozen and floating upon water. The thickened parts which stand higher above the general surface also project deeper into the liquid below. We have supposed the crust to have the specific gravity of granite, and the fluid substratum to have that of basalt. Hence their ratio is about 0·905, whilst the specific gravity of ice is 0·9176. These numbers are so nearly the same that the cases are exceedingly analogous, and the downward protuberance of the crust, as compared with the elevations above the surface, will agree closely with the immersed part of an iceberg as compared with the part exposed.

When the crust is crushed together along a mountain-range, part of the mass will be sheared upwards and part downwards. If it be equally rigid from top to bottom, half of it will go up and half of it down, and the neutral zone, as we have termed it, will be at half the depth. But, if the lower parts be softened by heat, a greater thickness will be sheared downwards than upwards. If the softening were to follow a certain assumed law, which would make it increase slowly at first but more rapidly at last, until it mingled with the fluid substratum, the neutral zone would be at the depth of one-third of the crust. This would be probably an excessive estimate. We accordingly have assumed a value between the two and place the neutral zone at the depth of two-fifths of the whole thickness.

And here we arrive at a stage at which we make our first attempt at estimating the actual thickness of the crust. Granite has been formed in the presence of liquid water. Water cannot remain a liquid at a higher temperature than 773° F., whatever pressure may be placed upon it. This temperature, at the rate of increase of 1° F. for from 51 to 60 feet, would be found at the depth of from 7 to $8\frac{1}{2}$ miles. Hence rock, which was once at the depth of from 7 to $8\frac{1}{2}$ miles, has been forced upwards; and therefore the neutral zone is not so deep as that. But if $\frac{2}{5}$ of the thickness were 7 to $8\frac{1}{2}$ miles, the whole thickness would be from 17 to 21 miles. Hence we conclude the whole

thickness is greater than this. Again, taking the temperature of melting slag at 3000° F., such a temperature would be met with at the depth of from 28 to 30 miles, and we can hardly suppose the temperature of the substratum to be so high as that of melting slag. Hence we place, roughly, the thickness of the undisturbed crust as a first attempt between these estimates, at about 25 miles.

Now supposing a tract of the crust crushed together by lateral compression; and that about two-fifths of the thickness goes up, and three-fifths goes down. If it were to remain in this position, we should have the ratio of the part above the effective level of the fluid to the part below it as 2 to 3. This would be impossible if it floated; just as it would be impossible that an iceberg should stand 200 feet above the water while only 300 feet were immersed. But the tract of crust does not exactly float; for it is held up to some extent by its attachment to the neighbouring crust. Nevertheless it cannot be held up long in what would be so constrained a position. It must then sag downwards; and the most thickened part would sink the most. Hence depressions would arise on both sides of the ridge, and the ocean, which covers the general surface, would be deeper than elsewhere along two channels parallel to, and at some little distance from, the ridge. But should the ridge be steeper on one side than on the other, as seems inevitable, the ocean would be deeper on the steeper side. This relative position of the depths of the ocean to mountain-chains is in accordance with nature.

Next suppose the ridge thus elevated to be denuded. The chief streams will be formed on the less steep side, and the sediment will go partly into the sea and be deposited along the shore line, and partly on to the lower lands. This will depress that portion, and it will sink; and the whole tract will be more or less tilted down on that side, and up on the other, because it will turn about an axis through its centre of gravity, which will be situated somewhere beneath the ridge. The ridge also, owing to the great downward protuberance, will stiffen the tract, and give it rigidity to bear this tilt. The

tendency will be for fissures to form, chiefly along the shore line on the steeper side of the ridge. The depressions, in which the deep water lies, will also by the same action be moved further away from the ridge on both sides.

XI. We apply to the downward protuberance of the crust into the substratum, under any elevated tract, the popular expression of "roots of the mountains." The existence of these roots of the mountains are not a mere matter of speculation. They have been felt by aid of the plumb-line in the following manner. The great mass of the Himalaya mountains was, during the Indian Trigonometrical Survey, found to attract the plumb-line. But upon its being calculated how much attraction ought to be attributed to this mountainous mass, it was found that, though they attracted the plumbline, yet they ought to have attracted it still more than they did. Sir G. B. Airy explained this anomaly by the existence of downward protuberances of a lighter crust into a heavier substratum; which is exactly the same supposition to which our reasoning has just led us. This then is a strong confirmation of our theory of the constitution and arrangement of the crust.

Another point to be observed is, that the floatation of a crust, thus dipping downwards into a heavier fluid at the places where it rises upwards into the air, precludes the transmission of any unequal stresses to the parts below, and renders unnecessary the supposition of extreme rigidity, either in the crust itself or in the subjacent matter, in order to support such stresses.

XII. Again, the existence of the roots of the mountains ought to be revealed by phenomena of underground temperature; and we find such to be the case. Whether the crust be thick or thin at any given locality, the temperature of its under surface ought to be the melting temperature. This temperature may practically be considered the same everywhere. Accordingly any difference in the rates of increment of temperature in descending at two localities ought to depend only upon the thicknesses of the crust, and upon the mean surface

temperatures; and the rate ought, as a rule, to be greater in lowlying than in elevated regions. This is known to be the case. Careful observations of temperature were made during the construction of the tunnel through Mont S. Gothard, and sufficiently reliable ones also at Mont Cenis. In both these tunnels the rate was found to be about 1° F. for 100 feet, which is only about half the usual rate.

The rate being known, and the height of the mountain, and also the rate at a place elsewhere near the sea level, if we assume the relative densities of the crust and substratum to be those of granite and basalt, we can calculate the thickness of the crust and the melting temperature from these data, without making any assumption about the melting temperature. In this way we have obtained for the thickness of the crust about 25 miles at the sea level; and for the melting temperature about 2500° F. The first of these values agrees with that already estimated in X., and the latter is by no means improbable from what we know about the melting temperatures of silicates. The calculations which we have made respecting the thickness of the earth's crust at the sea level, necessitate a corresponding thickness beneath the ocean. The result at which we have arrived is that, if the crust were of the same density there as beneath the continents, it would have to be thinner than is possible beneath the oceans. We therefore conclude that it is not of the same density beneath the oceans, and thus we are led, by another line of argument, to the same conclusion as that mentioned in VII., viz., that the crust beneath the oceans is denser, and that it is to this cause that the accumulation of water over one-half the sphere is to be attributed.

If we put the mean depth of the ocean at three and a half miles, with our assumed values of the densities of the crust and substratum, we have found that on these data the thickness of the crust at the sea level cannot be less than about 25 miles; which so far agrees with the estimates before made. And in this last calculation no considerations of temperature whatever are involved, as they were in the former, and have always been in previous estimates.

We conclude on the whole that the density of the sub-

oceanic crust may be about 2·955; that its thickness is there about 20 miles; while, in the continental areas, its density is about 2·68, and its thickness at the sea level about 25 miles.

XIII. We cannot explore the sub-oceanic crust, and do not know from direct evidence whether it has been compressed, as we know has happened to the continental areas. Many geologists feel assured that the oceanic areas have always been oceanic, and that they have never interchanged places with the continents. If that be the case they have never been compressed and elevated.

Referring to the two modes in which compression may have caused elevation, namely, by either raising the crust into anticlinals, into which the subjacent denser fluid would rise, or by producing ridges on the surface, which would be accompanied by corresponding depressions of the lighter crust, into the fluid beneath, we remark that, if the former were the arrangement, the two phenomena we have lately discussed would be absolutely reversed. For the attraction of mountain masses would be greater than if they consisted of matter of the mean density of the crust, instead of being less; and the increase of temperature would be also greater in mountainous regions, instead of being less.

Compression may have arisen from contraction of the interior, or from expansion of the crust, or from these two processes going on together. We have shown that the contraction of a solid earth through cooling cannot explain the existing inequalities. Neither does it appear that the contraction of a liquid substratum from mere cooling can have been much greater than that of a solid globe, for, before the time when a crust began to form, the liquid must have become reduced by convection to nearly the temperature of solidification; and subsequent cooling, accompanied by solidification and thickening of the crust, would have proceeded downwards, almost as if it rested on a solid nucleus, the loss of heat taking place from the crust alone.

In order to estimate the amount of contraction, which would have produced the existing inequalities on the hypothesis of a fluid substratum, we must proceed somewhat differently from

what we did under the hypothesis of a solid globe. This then we have accordingly done upon two suppositions, first that the crust beneath the oceans is 20 miles thick, and of the same density as beneath the continents, the oceans being supposed to lie in veritable depressions caused by the greater thinness of the crust beneath them. In that case we find that the radius of the globe, since the inequalities began to be formed, must have been shortened by about 700 miles.

This is a much greater shortening than can be supposed possible, and is an additional reason against the hypothesis of the sub-oceanic crust being of the same density as that beneath the continents. In the next place, we suppose compression to have been confined to the continental areas, which is equivalent to attributing the oceans to a denser crust beneath them. We have taken the thickness of the continental crust at the sea level at 25 miles; and in this case we find the shortening of the radius, which would produce the existing inequalities, to be about 42 miles; and then the horizontal compression of the continental areas would average about two per cent.

These results confirm the conclusion, that the ocean basins are not caused by depressions, in the upper surface only, of a crust of density everywhere uniform, but that they are due to a greater density, and general depression, of the sub-oceanic crust.

XIV. It being certain that a vast amount of water is given off in volcanic eruptions, we next examine the question whether, supposing all the water of the oceans to have been once beneath the crust, it would be possible to obtain a sufficient contraction of the globe to have produced the inequalities, by the transference of this water from beneath to above the crust. But we find that, upon the supposition, the most favourable that we can make, respecting the decrease of volume which the abstraction of this water would impart to the substratum, the hypothesis fails. For the entire remaining portion of the globe beneath the crust would not suffice to afford a sufficient amount of contraction to have caused the existing continental compression, under circumstances, more favourable than are likely, with

regard to the original addition of volume, which the presence of the water could contribute.

Changes in the earth's rotational velocity may have altered the oblateness, and compressed some parts of the surface and stretched others; but there does not appear to be any indication, in the arrangement of mountain chains, of their having been connected with such a mode of action.

XV. We have thus examined two probable causes of compression of the earth's surface, originating in contraction of the interior, namely, cooling, and extravasation of water-substance from beneath the crust. Neither of these appears to be adequate to produce the amount of compression which exists. No other cause of contraction of the globe suggests itself.

We next look about us for a possible cause of extension of the surface, which, it is obvious, would, equally with contraction of the interior, produce compression. The numerous more or less vertical dykes of igneous rock lead us to enquire, whether the rending of the fissures which they occupy, may not be indicative of the extension of which we are in search. The pressure of the crust upon the fluid substratum is about 10066 tons upon the square foot. If the water-substance from the magma were to escape into a crack in the under side of the crust, it would exert this pressure towards rending it wider; and so long as the vapour or gas was supplied with sufficient rapidity, it would prevent the magma from rising into the chasm so formed. When the rent reached the surface, the vapour would rush forth and be followed by the magma itself, now appearing as lava; and thus a volcano would be established. But this would be an exceptional occurrence. It would be only here and there that the vapour would escape at the surface, because its doing so at one point would relieve the internal pressure for a long distance.

The above-described process appears to be the mechanism requisite to establish a volcanic vent, and agrees well with the series of earthquakes which usually for a long while precede an eruption, and very often occur without any eruption at all. Nevertheless permanent elevation of the tract is said to be a common result of their action.

In order to arrive at some conclusion regarding the amount of force, with which the lava would be elevated in an open chasm, we have made a supposition respecting the constitution of the magma from which it is derived. Some late experiments by Mr Hannay, since published[1], appear to warrant our assumptions. We suppose the state of igneo-aqueous solution to be such as has been already described in VIII., and that diminution of pressure allows the water-substance to separate from the lava. This will rise through the column of lava and produce ebullition. But in our calculations we have omitted the consideration of this ebullition, and supposed the water-substance to remain in the identical mass of lava, simply expanding when separated from it by diminution of pressure. If the magma contains one-tenth of water in a given volume, it would be capable of thus rising to the summit of a lofty cone. And if a fissure was to be opened to the surface, the mean pressure of the lava upon the sides of it would be that of the weight of a column of rock about twelve miles high. This however would not be sufficient to produce compression, for which we must look to the pressure of the gas or vapour, confined in closed fissures, which at its maximum would be twice as great. The work of compression would be chiefly expended in deforming the materials of the crust, producing cleavage and so forth, the work of raising the mountains, and depressing their roots into the fluid substratum, being comparatively small. The ease with which masses of rock would move over surfaces of dislocation, once established and lined with slickenside, would appear to render regions, once disturbed, more liable than others to further disturbance. The quantity of igneous rock subsequently injected into the fissures would go to increase the solid crust in the disturbed regions, and, with the estimate we have already made, that the compression amounts to about two per cent., one-fiftieth of a vertical section through the crust ought to be occupied by dykes. But it must be remembered that these would be most numerous and widest in the lower side, so that much of them would never come under observation.

[1] See p. 276, and p. 277, note.

XVI. Compression is not the only kind of disturbance which has affected the rocks. Faulting is quite as common a phenomenon; and the amount of dislocation produced by it varies from a few inches to miles. We attribute faults to the formation of fissures, originating at the surface through contraction of the rocks, and propagated downwards. We attribute their usual hade to the downthrow, to the greater compressibility of the rocks on the downthrow side of the fault. Faults on the whole must tend to depress the surface of any tract dislocated by them.

Thus it will be seen that the two classes of disturbance which have affected the crust, are ultimately traced to the same exciting cause, namely the formation of fissures through metamorphic changes. When these fissures originate below and are propagated upwards, they become filled with elastic vapour and compression results, and when they originate above and are propagated downwards, faulting is the consequence.

XVII. The peculiar arrangement which is requisite for the equilibrium of a disturbed crust resting upon a heavier fluid substratum is, that, for every subaerial elevation above the mean surface, there must be a corresponding protuberance, dipping downwards into the fluid below; and according to the relative densities which we have assumed, the depth of these protuberances must be about ten times the height of the elevations. We have already seen that this circumstance explains two remarkable phenomena, connected respectively with mountain attraction and with underground temperature. We now proceed to show that it also agrees extremely well with many of the well-known geological movements of the surface. As the surface is denuded down, it will be concurrently raised up from below by floatation, and the immense quantity of material denuded off a tract will not require the tract at any one period to have had the altitude, which the amount of denudation at first sight appears to necessitate: nor yet will any additional exciting cause of elevation be required, beyond the mere fact of the denudation. The course of drainage across the dip is also explicable on this hypothesis.

XVIII. Volcanic energy is the motive power on our view of compression. It will be easily seen that we reverse the sequence of cause and effect, as the subject has been sometimes regarded. The well-known theory propounded by Mr Mallet, to account for volcanic energy, makes it the result of compression instead of the cause. It also removes the seat of it from where we believe it to reside. We therefore feel it necessary to reply to his arguments. Mr Mallet based his theory upon the results of experiments in crushing cubes of rock, from which he *calculated* the amount of heat obtainable in that way, and concluded that the whole of the heat, that would arise from crushing one cubic foot of rock, would fuse about one-tenth of a cubic foot of the same. Supposing the heat so obtained should be localized, he considered that the crushing of rock within the crust of the earth would originate volcanic action. He assumes with Hopkins, that the crust is thick, and postulates 400 miles for the thickness. We however show that the heat obtained could not be localized, but must be distributed through the crushed portion, appearing only where crushing took place, and appearing everywhere in proportion to the work done there; so that if say ten miles of the crust was crushed, it could not fuse one mile selected out of those ten.

We however do not leave the question here, but go on to calculate the utmost conceivable amount of heat obtainable under Mr Mallet's hypotheses, and find it quite inadequate for the purpose.

XIX. Having cleared the ground of the fascinating, but as we believe misleading, theory of Mr Mallet, we review theories that have been propounded to account for volcanic action, on the supposition that it is a manifestation at the surface of the intense temperature existing below; and that the lava and vapours, which are erupted, are brought up from an intensely hot substratum. Some of these theories require local increments of temperature, of which no explanation has been attempted; while others require the absorption of water by hot rock when applied to it externally. We believe that our theory of the constitution of the magma removes the difficulties

which are involved in the above suppositions, and we have made an attempt to explain the mechanics of an eruption, and the sequence of events connected with it. For the details of this part of the subject the reader is referred to the body of the book, since they cannot be very easily epitomized.

XX. The downward protuberances of the crust into the fluid substratum, which we have termed the roots of the mountains, will be gradually melted; and the material melted off, being lighter than the substratum, will flow upwards, and spread itself beneath the crust. This fact appears capable of explaining some hitherto not sufficiently explained phenomena, respecting the order of succession, in which different kinds of lava have been erupted. The Natural System of Volcanic Rocks of Richthofen, as generalized by Prof. Judd, asserts that, since the Eocene period, the earliest erupted rocks have been of an intermediate type, as regards the proportion of silica which they contain and their specific gravity. These were succeeded by more silicious lavas of less specific gravity; and finally followed by basaltic lavas, with the lowest proportion of silica and highest specific gravity. It is obvious that, if the couches of molten rock had been erupted in the order of their superposition, in which they must necessarily lie, those of intermediate specific gravity ought to have followed, instead of preceding, those of less. Our theory accounts for this circumstance. The more highly silicious rocks being solidified in the crust, were underlain by a magma of intermediate character, and, during the movements which raised the mountains and depressed the roots, this made its appearance at the surface as lava of intermediate type. Subsequently the depressed silicious crust was melted off, and underflowing the crust, the next eruptions were more silicious. When this layer, a thin one, had been erupted, or at distances from the ranges to which it had not extended, the denser substratum followed next in order, producing basaltic lavas.

The theory of mountain roots also accounts for the fact mentioned by Prof. Judd, that the ejections in regions, lying upon opposite sides of a mountain chain, appear to have come

from different reservoirs of molten rock: for according to our view this would be actually the case.

Thus we find five classes of phenomena explicable on the theory of mountain roots:

(1) The support of the mountains themselves,

(2) The apparently insufficient attraction of the Himalayas,

(3) The slower increment of temperature beneath mountains,

(4) The gradual elevation of a tract concurrently with its denudation,

(5) The natural sequence of volcanic rocks.

XXI. The geographical distribution of volcanos presents fewer difficulties upon the supposition of a thin crust and fluid substratum, than upon any other. Their linear arrangement points to their being situated along great systems of fissures; and such systems of fissures are indicative of a thin crust. Fissures, which run for long distances in nearly straight courses, point, either, as already mentioned when discussing faults, to a movement perpendicular to the fissured surface, or else to a rending pressure within the fissure itself; while on the other hand fissures, which are caused by contraction in a direction parallel to the surface, would divide up an area into polygonal figures. The former arrangement of the fissures accords best with the distribution of volcanic ranges, and suggests a thin crust.

We recognise two principal types of volcanic regions, coast-line, and oceanic. We believe the former to be connected with the agencies which have raised the continents which they skirt. Trains of vents are attached laterally to the great compressed and elevated ranges, and usually stand near the edge of a steep shore. The oceanic volcanos on the other hand appear unconnected with true elevatory action, for the oceanic islands consist almost all of them of volcanic rocks: whereas, if they were connected with areas of elevation, the peaks of schistose or other hard inclined strata could not well be absent.

Volcanic cones, having no roots projecting downwards into the substratum to support them, would sink, and rupturing the crust around them, tend to perpetuate volcanic activity in the same region. The fissures would be formed around the cones, and thus a train of volcanos, originally linear, would give occasion to an elliptical ring of vents, to which the peculiar shape of some of the coral archipelagoes may possibly be due.

The existence of some kind of connection, between volcanic action and the elevation of continents, is indicated by the remarkable course of the great volcanic band which borders the Pacific ocean: for it approximately divides the world into two hemispheres, one of which contains nearly all the land, and the other, except Australia, is covered with water. But the course of this great band being turned aside by its junction with another, which comes from the N. W. through Sumatra and Java, the exceptional continental area of Australia has been elevated within the oceanic hemisphere, in connection with the thus abnormally deflected continuation of the great band.

Our theory of volcanic action needs only the opening of fissures which shall reach the fluid substratum, without reference to how those fissures may be caused. We think therefore that the present localization of volcanic activity, at interrupted regions along the flank of an elevated range, may be traced to the deposition of sediment, even at a considerable distance, upon the side of the range opposite to that on which the volcanos lie. An inspection of our diagram on page 132 will explain how this would be: and there appears some reason to suppose that this suggestion is supported by geographical facts.

APPENDIX.

Mr G. H. Darwin "On the stresses caused by continents and mountains"—his argument for solidity met by Sir G. B Airy's explanation—M. Roche "On the constitution of the interior of the earth"—his theory, though in accordance with Geology, requires fuller statement than is yet published.

NOTICES of two yet unpublished memoirs have lately appeared bearing on the Physics of the Earth's Crust. Mr G. H. Darwin read a paper in June, 1881, before the Royal Society, "On the stresses caused in the interior of the earth by the weight of Continents and Mountains[1]." It is based upon the hypothesis that "the existence of dry land proves that the earth's surface is not a figure of equilibrium appropriate for the diurnal rotation. Hence the interior of the earth must be in a state of stress, and as the land does not sink in, nor the sea-bed rise up, the materials of which the earth is made must be strong enough to bear this stress." After describing the nature of the investigation, he concludes that it "must be regarded as confirmatory of Sir William Thomson's view, that the earth is solid nearly throughout its whole mass. According to this view, the lava which issues from volcanoes arises from the melting of solid rock, existing at a very high temperature at points where there is a diminution of pressure[2], or else from comparatively small vesicles of rock in a molten condition."

[1] "Proc. Royal Soc." Vol. XXXII. p. 432. 1881.

[2] See the Author's paper on "The Elevation of Mountains by Lateral Pressure." *Trans. Cambridge Phil. Soc.*, Vol. XI. Pt. III. p. 504. 1871.

It will be seen that the facts, upon which Mr G. H. Darwin bases his investigation, are the same as were perceived by Sir G. B. Airy to require explanation. Admitting that the materials of the earth cannot be strong enough to bear the stress, the difficulty was explained by him in the manner given in his own words in our eleventh chapter. If that explanation be accepted, the stresses arising from the inequalities of the surface can exist only within the elevated tracts themselves, which possess no doubt sufficient rigidity to keep them from flowing down into flatness. If the crust is of appropriately varying thickness in different parts of the land surfaces, and denser beneath the oceans, and floats in equilibrium on a fluid substratum, it cannot transmit any unequal stresses to the subjacent parts.

The other notice referred to appears in the *Comptes Rendus de l'Académie des Sciences*[1], and is by M. Ed. Roche. He points out that "the conditions which every hypothesis ought to satisfy respecting the distribution of the interior mass of the earth are, that it should agree with the value of the superficial ellipticity and also with a certain constant depending upon the phenomenon of precession. These conditions are very nearly satisfied upon the hypothesis of fluidity if we admit that the ellipticity of the earth is about $\frac{1}{300}$; but if the ellipticity is higher than $\frac{1}{295}$, as appears to result from more recent determinations, the agreement no longer exists."

"There is then room to undertake these investigations again upon a different hypothesis; for example, by considering the globe as formed of a nucleus or solid block nearly homogeneous, covered by a lighter layer, whose density from geological considerations may be estimated at three times that of water. This constitution of the globe being premised I find that it is possible to reconcile the actually admitted values of the precession and ellipticity if we take account of the fact that the interior nucleus of the globe is solidified and has received its definite form under the influence of a rotation less rapid than that with which the earth is actually animated."

[1] Tome XCIII. No. 8 p. 364 (22 Août, 1881). Paris.

"In any case, the contraction due to the cooling of the globe ought to introduce a progressive acceleration of its angular velocity. But, if this globe is fluid, the form of the different layers adapt themselves continually to the rotation such as it is at every instant, so that, finally, there remains no more trace of the successive changes which their ellipticity has undergone since the beginning. If on the contrary at a certain epoch of the cooling, the interior layers have passed into the solid state, these layers have taken and retain an ellipticity very different from that which the general hydrostatical equation would attribute to them, applied to a mass entirely fluid possessing a rotation common to all its parts. The formulæ calculated on the hypothesis of the solid nucleus contain at once the constant q, the actual ratio of the centrifugal force to gravity at the equator, and the value q_0 of the same ratio at the epoch of the solidification of the central block. This last element not being determined, we may give it a value such that the superficial ellipticity agrees with the coefficient of the precession. It is necessary, for this, to suppose q_0 less than q, whence it results that the rotation of the earth has undergone an acceleration since the consolidation of the interior nucleus."

"The physical and astronomical conditions of the problem allow us moreover to determine with sufficient precision the dimensions and the specific weight of this block. If we neglect the purely superficial shell, as well as a slight condensation towards the centre, where the heaviest materials must be collected, observe what will be the constitution of the globe: a nucleus of which the density is about 7, covered with a layer of density 3, of which the thickness will not attain $\frac{1}{6}$th of the entire radius."

"The terrestrial block is then as regards the specific weight, analogous to meteoric irons, whilst the layer which envelopes it is comparable to the aerolites of a stony nature, into which iron enters only in small proportion."

It need hardly be said, that the results obtained by M. Roche are such as most Geologists would be ready to accept. Until the memoir appears in full, it is not possible to form a

decided judgment upon the argument. But it will be observed that the notice makes no mention of the amount of contraction of the spheroid, which would be required to give the additional velocity of rotation. This is a very important point; for it must be remembered that, while contraction is accelerating, tidal friction is in the meanwhile retarding the rotational speed[1]. It is hardly conceivable that the supposed central block could have solidified very long before a crust could form, and the results of our present investigations tend to show, that the contraction since that epoch has not been great.

[1] The remarkable and profound investigations of Mr G. H. Darwin, upon The remote history of the Earth, and on The Tides of a viscous Spheroid, will be found described in a popular manner by Prof. Ball, in "Nature," Vol. 25, p. 79, 1881.

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